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# JOURNAL OF SCIENCE.

AUGUST, 1880.

## I. REPORT ON SCIENTIFIC SOCIETIES.

By Dr. C. K. Akin.

OUR readers are doubtless aware that some years ago there was a formal inquiry as to the most efficient means for the advancement of Science in England. The following letters on this important subject were addressed to Professor Stokes, Secretary of the Royal Society, by a gentleman well acquainted with the universities and learned societies of most parts of the Continent. They have never been published, and as they have been kindly placed at our disposal by the author we gladly embrace the opportunity of laying them before our readers.—Ed. J. S.

*Pesth, June* 24, 1870.

My Dear Sir,

As the terms of appointment of the Royal Commission of which you are a member make reference to England only, perhaps you were astonished at my wishing to tender it any advice. But, on consideration, you will no doubt agree with me that, Science being in its essence cosmopolitan, whatever concerns the science of any nation cannot but awaken the interest of scientific men at large, of whatever country. You will also recollect that, in the course of a more than three years' residence, in the aggregate, at Cambridge, Oxford, and London, I have had some experience of the state of Science in England; which I have been able to compare with the corresponding state of things in Germany and in France, having spent, both previous and subsequent to my stay in England, seven or eight years on the whole at

Berlin, Göttingen, Heidelberg, Königsberg, and Paris. My motive for stepping thus forward is the hope of being possibly instrumental in effecting some good; but I freely confess also to the desire, which I am apt to entertain like many other persons, of discharging my mind upon a fitting occasion of thoughts which have weighed for years upon it; and, with regard to the matter on hand, I may perhaps mention that my meditations concerning it date as far back as my experiences.

I intend to divide the remarks I shall make under four heads, having reference to—1, the composition and functions of scientific societies; 2, the efficiency of the universities and kindred institutions with regard to scientific instruction and research; 3, the services of scientific literature of a periodic and non-periodic character; and 4, the recognition of Science by Society and the State.

In this present letter I shall confine myself to a consideration, very summary, of the subject first mentioned, viz.:—

*Of Scientific Societies.*—The scientific societies extant in England may be distributed among four classes: the *first*, containing the Royal Society, so-called κατ' εξοχην; the *second*, the remaining London Societies; the *third*, the British Association; and the *fourth*, the Provincial Societies. If it be now asked, what are the functions of these several associations, and in what do their aims differ or agree? the answer will prove a little embarrassing, and, if candidly given, not over satisfactory.

As regards the Royal Society, there was undoubtedly a time when it proved of great utility to Science. Its Transactions were the *Acta eruditorum* or *Journal des Savants* of England; its Secretary corresponded with many of the foreign *virtuosi*, acting thus as a connecting-link between English and continental philosophers; finally, its Curator repeated and made known all the newest facts and experiments at the Society's meetings. That was in the early stages of the Society's existence, when it also kept a laboratory of its own; but now all this is changed. The Society still publishes its Transactions, containing highly valuable papers; but it thereby confers no boon upon Science, as many of the papers are reprinted, and in fact all might be published, with greater despatch and in a more accessible form, through the means afforded by the scientific periodicals. The office of Secretary still exists, and there are even three secretaries acting at present, one of whom is specially designated as the Foreign Secretary; but their functions bear no trace of what they originally had been. The curatorship,

on the other hand, as well as the laboratory, have ceased to exist.

In consequence of the great subdivision of Science the meetings of the Royal Society are attended by few members, still fewer of whom generally take any interest in the papers that are read. As for the medals awarded by the Society they can be considered only as badges, but scarcely as recompenses, still less as incentives to work. Similarly, the sums annually distributed out of the donation fund, and the grant obtained from Government, have been hitherto productive of little good, at least in so far as they have mostly conduced to such researches only as without such aid would have been executed as well; and strange to say, the fund and grant, however unimportant, are not even annually exhausted. The greatest service that Science has recently had to thank the Society for consists in the publication of the Catalogue of Memoirs which has been undertaken by the Royal Society, partly, upon the initiative of the British Association, and at the expense (so far as printing, paper, and binding) of Government.

I trust that neither yourself as Secretary, nor anyone else, will consider the above remarks as conveying anything disrespectful to the Royal Society, which, from the great number of highly eminent persons it includes, cannot but command, even if it had no other titles, everybody's esteem. But if I may quote words of my own, "a Society may be said to fulfil a useful purpose if its members combined produce a greater amount of valuable results than they would in the aggregate if in a state of isolation; or, still better, if the Society in its corporate capacity performs services which no individual or individuals unconnected with each other could render." Now, I may be permitted to ask, has such been the Royal Society's case of late years? In my opinion it is more of an ornament than a utility to the State; and the tendency to make it such is apparent also in the comparatively new practice, according to which the number of Fellows annually elected has been limited, and that at quite an inconsiderable figure. Such a proceeding is intelligible enough, although detrimental likewise, in the case of the foreign academies, the members of which are paid, and considered somewhat as functionaries of State; but it must appear strange on the part of a voluntary association, the members of which pay an annual contribution, destined for the "promotion of natural knowledge."

The remaining London Societies—being formed on the model of the Royal Society, but less exclusive in their elec-

tions, although more narrow in the circle they embrace—enjoy upon the whole a greater popularity and show more signs of activity. Still, although at times lively, their meetings produce in the end nothing, or scarcely anything, but papers, the publication of which is the only observable sign of their existence. To this latter subject, however, I propose to revert again when I shall come to discuss the state of scientific literature.

The British Association is in part like to the Royal Society from the number of sciences it embraces, but in part also it is similar to the other scientific societies, admitting, as it does, all and any persons who are willing to pay.

It also resembles the Provincial Societies—concerning which nothing in particular need be said—from its holding its Sessions in provincial towns; and in sowing thus broadcast the seeds of Science all over the country may be said to consist of the principal service rendered by the British Association. For as regards its reports and grants, the same remarks might be applied that I have made above; and as for the Observatory kept at the Association's expense at Kew, it is not clear to me for what reasons its objects could not be attained at Greenwich as well.

The defects I have noticed in the working of the scientific societies arise, in my opinion, from the want of a proper organisation of each, and the absence of any co-operation among themselves. As a remedy, I propose that some kind of connexion should be established between the several societies and the Royal Society, on the footing of the affiliation that subsists between the colleges and the university. Every chartered society should be represented on the council of the Royal Society, which would thus become a kind of metropolitan board of science in permanency. In the same way the London and the provincial societies should be represented by delegates on the council of the British Association, which, thus constituted, might be considered as a kind of national synod of science, holding an annual session of short duration. I do not propose this organisation, of course, for its own sake, but as a means to several ends, the nature of which I shall explain in subsequent letters. For the present it will be sufficient for me to observe that the scheme I propose will scarcely require any change, although it implies considerable innovation; and I apprehend there will be little difficulty in carrying it out, if once it be recognised as useful, from the fact that both the Royal Society and the British Association contain already the greater number of the more influential members of the

several London and provincial scientific societies. But should there any difficulties arise,—such as impeded the union that it was once intended to establish between the Geological Society, I believe, and the Royal Society,—then it will be a case for the Royal Commissioners to step in, as was done in the instance of the colleges on the occasion of the university reform.

Requesting of you the favour to communicate this letter to your fellow Commissioners,

I remain, my dear sir,
Yours very sincerely,
C. K. AKIN.

To G. G. STOKES, Esq., M.A., Sec. R.S., one of the Royal Commissioners on Scientific Instruction and the Advancement of Science, London.

---

*Pesth, June 26, 1870.*

MY DEAR SIR,

In the present letter I shall consider the efficiency of the instruction and aids to research derived—

*Of the Universities and kindred Institutions.*—As for the universities, my observations have reference only to Oxford and Cambridge, which I know by personal experience; and as for the other institutions, I shall limit my remarks to the Royal and the London Institutions.

To the minds of the uninitiated, especially of the uninitiated foreigners, an account of the English Universities, of their colleges and foundations for professorships, fellowships, scholarships, and the like, must appear as the realisation of the ideal institution which is to ensure the perpetual advance of science and learning; yet when we come to look to facts, however useful colleges may have proved with regard to education, their tendency has not been to foster any such scientific activity as that of which their several foundations seem to hold out the prospect. Perhaps it might not be uninstructive to count up the number of modern English scholars, philosophers, and thinkers who have lived, or still live, without the pale of the universities; and it will be contested by no one that, in Germany, at least, the universities *are* in a much higher degree centres both of national and European thought than the English Universities were ever *asserted* to be by their best friends or warmest admirers.

And if England be still foremost among nations in most branches of Science, it is owing mainly to that private and individual energy of mind which distinguishes the English race beyond any other, and in a much less degree only to the corporate action of the universities or to their system. In Germany there are no foundations to secure "a learned leisure" to the successful student, and no mitres to crown the careers of eminent scholars and mathematicians; moreover, within their juridical, medical, and theological faculties, they afford all that professional instruction of which the English Universities—in my opinion, properly so—are absolved, either wholly or in part, by the inns of court, the hospitals, and certain theological colleges. Yet, notwithstanding that defect on the one hand, and the practical bias thus imparted on the other hand, the eminence in, and devotion to, abstract science and pure learning of the German Universities is actually in Europe unsurpassed.

The causes of the differences noticed in the comparative working of the English and German University systems appear to me to be *two* in number. In the *first* place, the English Universities are much less considered as "seats of learning" than as "seats of education," if I may use an expression actually applied by Mr. Gladstone to them. In Germany the *general education* of youths is supposed to be finished at those establishments which, by the age of their pupils, if not by their constitution or systems of teaching, correspond to the English Grammar and Public Schools. At the University the *instruction* is either *special* or *professional;* special in the case of those who make Science their calling, and who frequent principally the so-called philosophic faculty; and professional in the case of others (the majority) who wish to become doctors, barristers, lawyers, judges, or priests. Teachers at the German Universities, instead of doing the work of schoolmasters,—which is rarely compatible with the prosecution of independent research,—form "schools," in the higher sense of the word, in which a tradition is handed down from individual to individual and from generation to generation.

All this is different in, or absent from, the English University system. Even at Cambridge, the mother university of so many famous mathematicians, every fresh mathematical inquirer has to set up for himself and begin *de novo;* for the tradition which there exists relates rather to the modes of tuition than to any methods of investigation. And this brings me to the *second* point I wished to notice, as one of the main causes of the comparatively small influence the

universities have exerted on the growth of Science in England. In consequence of the competitive system adopted at the examinations, tuition at the universities has developed into a species of "cramming" *of a very high order*, or, as it may perhaps better be called, a species of "drilling" rather than of training of the mind. As a further consequence, the mental grasp of a high wrangler is generally quite astonishing, and he is capable of solving, so to speak, any problems that may be set him; but he has mostly lost that inventive faculty which busies itself about the problems implied by Nature, and the very existence of which must be divined. Mostly, great philosophers have worked at questions which *none* had formulated before them; and many have declared their inability to work at others, or shown their incompetency with regard to such subjects as had not been objects of their predilection. To force a person to inquiries of *any* kind may be a commendable proceeding in the case of individuals whose callings will bring them in contact with the problems of the practical arts, or generally of life; but neither discoverers nor inventors of a high order can be bred in such fashion. And, as long as fellowships shall be considered as prizes to be obtained only at competitive examinations, it is not to be expected, in my opinion, that the rich foundations of the universities will bear that fruit to Science which they might or ought to; although "Science," in the more limited sense, be admitted in future on the same footing at the examinations as "classics" or "mathematics." The exhaustive strain on the mind produced by these examinations, or the preparation for them, is another element to be considered, and which induces Fellows to repose rather on their laurels than to exert themselves in any further fatiguing work.

Probably a greater part than to the universities is to be ascribed in the spread and development of modern science to the Royal Institution, which has been the scene of the teaching and labours of the three by far greatest philosophers of our century,—of Young, of Davy, and of Faraday,—for neither of whom was there, nor for the two latter of whom could there be, any room at the universities. But, however valuable and important the results achieved by these extraordinary men may be, and although other distinguished persons have worked by their side or may have acted, in one or other case, as the successors of either, the Royal Institution cannot be considered as a school or seminary of philosophers or scientific men. Such it was not even its original object to be; that being, if memory deceive me not, very

different, as conceived by Sir Benjamin Thompson (Count Rumford) and the other founders, from what it has actually turned out, and still more different from any such design as has above been mentioned.

The object chiefly aimed at at present is the diffusion of scientific knowledge, which, if I mistake not, is also the avowed, and almost only, object of the London Institution. Perhaps it should be stated that, as far as Science is concerned, the beneficial effects of this popularising tendency, chiefly promoted by the establishments under consideration, are not always unmixed with evil; for, as was once remarked to me by a person whose name I need not recall to you, "Popular lectures are more frequently intended to make people believe they understand Science than to make them actually understand it." And if once a lecturer, instead of aiming to elevate and expand the minds of his hearers, contract the habit of narrowing Science itself to the degree of exiguity supposed to correspond to the general hearer's intellect, and thereby disfigure and debase it, Science frequently becomes with him, also in his capacity of inquirer, an object for show and exhibition, rather than for disinterested research and truthful exposition.

In the proposals which I shall now submit I am chiefly guided by the following considerations, which I have already had occasion to state in print:—"It is especially by encouraging those who travel along some other than the beaten track of ordinary men of science that corporations representing the interests of science might tend to promote it. . . . It is true that there are not many persons answering to this description to be found at any particular time [many more, however, than one naturally becomes aware of]. It is the exclusive glory of this country to have produced men like Davy and Faraday, for instance, for whom there would have been absolutely no room abroad to develope their talents; yet a little less constancy on the part of Mr. Faraday, if biographical notices inform us aright, would have deprived the world of a discoverer as unrivalled and prolific in his line as Newton was in his; and who knows but that the cold shade of neglect, the want of due encouragement extended to merit exhibited in some unusual way, may not be nipping every now and then some great discoverer in the bud? . . . A German physician still living [and *now* heaped with useless glory], who is now acknowledged to have been among the first to recognise one of the most important laws in natural philosophy, encountered so much annoyance and vexation whilst endeavouring to bring his discovery before

the public in a manner to ensure the attention of men of science, that he absolutely went out of his mind. . . . In the same way many a promising suggestion happens to be advanced by persons who have either not the leisure, the convenience, or the instruments to verify it by experiment, and is therefore totally disregarded"—or else dishonestly appropriated by unscrupulous persons, to the detriment of the discoverer, and thus indirectly of Science itself.

Starting from these principles, and considering that the universities are chiefly engaged in the *general tuition* of Science, while the kindred institutions mainly aim at its *popular diffusion*, I believe there is room for the creation of an establishment the object of which shall be the *special advancement* of Science, both by instruction and research. The inner organisation and outer relations of this establishment, which, for brevity's sake, I will call the Royal College, I think should be based upon the following foundations and governed by the following rules:—The members of the college should consist of probationers, fellows, deans, and president or master. As *probationer* any person should be admitted who could satisfy the council of any chartered scientific society, under the restriction to be later apparent, of his willingness and capacity for scientific work in the department being of such society's competency. After the lapse of one year, and on presentation of a report by the competent dean to the council of the Royal Society, which shall be judged by it satisfactory, the probationer will be admitted as a *fellow* into the college. During his stage of probation, the person introduced to the college by any society shall have his expenses for apparatus, &c., defrayed by such society (as also his personal expenses, if the society be so inclined); on being, however, admitted a fellow, he shall be both personally maintained and furnished with all necessary requisites for work at the cost of the college. As *deans* shall be elected such of the fellows of at least three years' standing as by their labours in the latter capacity shall have given most satisfaction to the council of the British Association, that shall be enabled to take cognisance of each fellow's work; it being understood that both the councils, that of the Royal Society above mentioned as well as that of the British Association, shall have been so constituted as stated in my former letter. Fellows should be nominated for three years, but capable of re-appointment; and the deans also, each of whom shall represent, singly, one distinct department of science, should be elected for the same term, but not be capable of re-election, the functions

of dean, as to be explained below, requiring a constant infusion of fresh blood. The president or *master*, being the administrator and governor of the college, shall be nominated by the Crown, upon a joint presentation of the two councils of the Royal Society and the British Association.

The duties of probationers and fellows shall be simply to work, the former in order to be admitted as fellows, the latter in order to be re-appointed; those of deans, on the other hand, will be of a twofold nature. In the first place, it will be their office to aid and supervise in their work both the probationers and fellows, each of his own department; and, secondly, each dean will give in each year or session one course of lectures, the subject of which shall be chosen by himself, but varied from year to year. It is supposed, moreover, as evident of itself, that it will become the dean to work also on his own account in order to be re-elected in his capacity of fellow, which shall remain independent of that of dean. The duties, lastly, of the master, as already referred to, shall consist mainly in the government and administration of the college, as well as in its representation in all external relations.

I now come to the chief question, How shall the expense of this establishment be defrayed? According to my views the British Association, the Royal Society, and the principal chartered societies, firstly, should tax themselves to a certain amount annually, in order to realise a scheme which I cannot but believe is in harmony with and will greatly promote the tendency of them all, viz., the advancement of Science. In what manner, next, the Government certainly, and perhaps also the universities, ought to be induced to contribute, both to its maintenance and foundation, is a subject for the Commission to deliberate upon. I will only further state that I think the college might perhaps be suitably connected with the British Association's establishment at Kew; and that, besides laboratories, it should contain all the necessary adjuncts for living furnished by colleges at Cambridge or Oxford. Should the universities be willing to partake in the erection and maintenance of the Royal College, of course they ought to enjoy the same rights of presentation as awarded to the chartered societies, as also some influence in other appointments and elections.

Believe me, my dear sir,

Yours sincerely,

C. K. Akin.

Prof. Stokes, Sec. R.S.

PS.—Of a not quite unimportant function the Royal College, by its members, will be able, and made, to fulfil with regard to scientific literature, I think it preferable to speak in my next letter, in which the latter subject shall be treated as a whole.

C. K. A.

---

*Pesth, June* 28, 1870.

My Dear Sir,

Conforming to the order adopted in my first letter, in enumerating the subject matters to be discussed, I shall treat in this present letter—

*Of Scientific Literature, Periodic and Non-periodic.*—The class of works which comes here under consideration is composed of *three* different categories: the first comprising the works intended for specialists or scientific men exclusively; the *second*, those designed for amateurs, if not wholly yet principally; and, finally, the *third*, those written simply for popular readers.

To the category *first* mentioned belong, independently of treatises and similar works, the manifold scientific journals, periodicals, proceedings, and transactions, published partly by booksellers and partly by societies. Now nothing can exceed the unsatisfactory state of this latter species of literature. A physicist, for instance, in order to become familiar with the current progress of his science, has to consult some twenty different works published periodically, yet at times more or less uncertain; but if he be a little more scrupulous or comprehensive in his tastes, and have a mastery of other languages besides English, German, and French, which in the above estimate I have alone taken into account—then, instead of twenty, there will be even two or three times twenty such works to call for his attention. The statement holds as good for most other sciences as it does for natural philosophy; and in one or two branches only has there recently been any attempt made to *condense*, rather than to concentrate or *centralise*, all new publications scattered periodically through a great variety of works and bearing upon some distinct portion of science. But, without wishing to detract from the usefulness of these undertakings, it may be asserted that they rather indicate than supply a want.

As far as the inconvenience just referred to goes, men of science are the only sufferers; but there is another perplexiity

to which they are liable in common with the important class of amateurs, and which is owing to deficiencies in the *second* species of literature before adverted to. Next to the desire for complete information concerning his special department, every man of science is, or should be, animated also by the wish for some such knowledge of the progress achieved in all kindred departments as would satisfy, ordinarily, the simple amateur. More than one attempt has been made already to supply the want here stated, but each has hitherto ended in failure, excepting the one last made, which is highly to be commended, but is yet not, perhaps, quite sufficient. In fairness it should be stated that the task of carrying on a publication of this kind, so as to be successful and complete, may almost be called formidable.

The *third* and last category of books may here deserve also some words of notice. In the introduction to his work on natural philosophy, Dr. Young has made the following observation :—" I shall esteem it better to seek substantial utility than temporary amusement, for if we fail to be useful for want of being sufficiently entertaining we remain at least respectable ; but if we are unsuccessful in our attempts to amuse, we immediately appear trifling and contemptible." Such are not the views acted on or adopted by many modern popular writers. While the most abstruse questions of politics, for instance, are daily discussed, not in treatises only, but in newspapers even, in a becoming manner, popular authors on Science, both in periodicals and independent works, generally address the public in a style befitting rather the intellect of children than that of grown-up people, for whom those yet are designed. Even in books calculated for well-educated persons, the matter is frequently dressed up in the fashion generally deemed suitable only for "the million " in other departments of literature ; and it is impossible not to be astonished, if we once come to reflect upon it, at the amount of conceit shown in this way by authors and tolerated by the public. It seems almost as if it were agreed upon all hands that Science is as much above the comprehension of ordinary men as Politics are supposed to be above that of ordinary women ; and as if men in the one case, like women in the other, concurred themselves in the verdict.

In order to remedy the defects noticed in the first two species of literature (for with the third I am here, so far, not concerned), I propose the following scheme :—

In the *first* place, all scientific societies of any importance, in England as well as in foreign countries, should be invited

to publish their transactions in such form as to make each single paper contained in them purchaseable *separate*. From all such societies should be obtained also a limited number of copies of their complete Transactions, the several papers published in which shall be collected and bound up in volumes corresponding, as far as their *matter* goes, to the various branches of Science. Such volumes might be issued annually, one for every department, and bear the title "Contributions to Knowledge," with indication of the year and the name of the science.

The measure just explained takes into account current literature only; but even greater difficulties attach to the accumulated store of works published in the course of centuries or ages, if information concerning their contents be desired. As you stated last year at Exeter, "To make oneself, without assistance, well acquainted with what has been done, it is requisite to have access to an extensive library, to be able to read with facility several modern languages, and to have leisure to hunt through the tables of contents, or at least the indices of a number of serial works." And as you went on to say, "With a view to meet this difficulty, the British Association has requested individuals who were more specially conversant with particular departments of science to draw up reports on the present state of our knowledge in, or the recent progress of, special branches." Such, however, is the labour involved in the preparation of such reports that all the influence of the Association has *not* been sufficient to induce a number of scientific men to *regularly* undertake it; and as it is found in practice that mostly young persons only have sufficient enthusiasm for labour of this kind, so the British Association has shown only in the early stages of its existence much activity in this direction. A most valuable surrogate for reports of this kind will be furnished by the great Catalogue of Memoirs published by the Royal Society at the British Association's instigation; but more than either reports or a mere catalogue is required in order to satisfy our want for some synopsis of the work done in the various branches of science.

I therefore propose as a *second* measure, and co-ordinate with the above, the publication at times of volumes comprising collections of all papers published within a stated period, and having reference to some such branch or question in science as has prominently sprung into notice or is rapidly progressing; for concerning such, as you observed, it is by no means easy now to get acquainted with its actual state, nor, as I will add, with its history. The papers should be

communicated either as a whole, or in such abridgments as will bring out their importance in respect of the matter treated. In all cases the author's own words ought to be used, and perhaps they should be given also in the original language; for it is not so much the diversity of languages as the multiplicity of works familiarity with and access to which is required that is the great stumbling-block in the way of getting up any question. This publication might bear the name of "Archives of Science," with sub-titles corresponding to the subject-matter of the volumes. As an annex or supplement to this collection might be published also, from time to time, volumes comprising the collected works of those philosophers whose merits in science would deserve such a memorial of their work. As belonging to this part of the subject, and preparatory to the above serials, I would suggest the speedy publication of indexes to such works, periodically issued, as now lack them; and among which even those important collections the "Philosophical Transactions" and "Philosophical Magazine" must be numbered, at least in respect of their latest series.

In the *third* instance I propose the publication of periodicals to be issued monthly, each devoted to a distinct branch of science, and the objects of which I can most shortly explain by a statement of what should be their uniform table of contents, viz., (1) original communications on new work; (2) reprints or abstracts of papers published in journals or proceedings; (3) notices of books and bibliography; (4) correspondence and discussions; and (5) miscellaneous. These publications, which I will call *bulletins*, should be like the Archives polyglot, for the reasons above stated and in the manner there explained; actually the existence of a periodical of this kind (the "Astronomishe Nachrichten"), which stands high in public estimation and usefulness, proves the practicability of the principle. But in connexion with the *bulletins* should be issued another serial, and this *wholly* in English, to be published weekly, and giving a summary of progress in science and other intelligence, culled from the *bulletins*, and adapted for the amateur or cultivated general reader who takes some interest in science. This publication should contain (1) leaders on scientific questions of the day; (2) abstracts, reprints, or translations from other journals; (3) proceedings of scientific societies; (4) reviews of books and bibliography; (5) news and intelligence; (6) complete lists of scientific papers published in periodicals. This publication, which has its near prototype in an extant periodical, might be designated as the "Journal of Science."

With regard to the realisation of these several projects, I believe that the British Association might well take the initiative in respect of the *first*, or the formation and issuing of the contributions; while the *second*, relating to the collection and publication of the Archives, might be carried on under the auspices of the Royal Society. The composition and editing of the Bulletins and Journal, *finally*, should form part of the duties of the Royal College. The expense entailed by these publications, in so far as it may not be covered by their sale, should be borne by the corporations mentioned and the chartered societies jointly, or eventually by the State, as is done in the case of certain publications now issued in the interests of history. The universities also might help by lending the aid of their presses.

Should the suggestions here made be adopted and acted on, England would confer benefits on science and scientific men all over the world; but at the same time it would set an example which other countries would be sure to follow, and would start enterprises in the costs and glory of which other nations, no doubt, would hasten to share, if the stimulus were once given. Actually, international societies have been formed for various purposes, some even for strictly scientific objects—as in the case of the International Association of Astronomers; and there is no reason why those beneficial institutions should not be extended and more largely adapted to science, so as to make all nations work, either by congresses, conferences, or associations, for objects which are common to them all.

In conclusion, I will only state that, of the various proposals made in these letters, those above developed I have perhaps most maturely considered, and with their importance I am most deeply impressed, as they have engaged my attention and thoughts earliest and oftenest. Without a knowledge of previous researches, as you said last year at Exeter, "there is always the risk that a scientific man may spend his strength in doing over again what has been done already, whereas with better direction the same expenditure of time and labour might have resulted in some substantial addition to our knowledge." And, if it be the primary object of Science to seek and to find out truth, to preserve and rescue truth from oblivion is to her progress of no less vital importance, as all must agree.

Mentioning, finally, for your information, that my next letter will be the last of the series,

I remain, my dear sir,

Yours very sincerely,

Prof. STOKES, Sec. R.S. C. K. AKIN.

(To be continued.)

## II. THE CONSTITUTION OF THE EARTH.

By ROBERT WARD.

THE notice which was given, in the "Journal of Science" for June, of my book, "The Constitution of the Earth," well deserves my thanks as an honest attempt to give a fair idea of its contents. A book which, as the reviewer observes, is "strangely at variance with the teachings of our received scientific text-books," might have been safely described in garbled language which appealed to the prejudices of the reader, and the writer might have triumphantly retired from his work with the proud consciousness of having annihilated the humble individual who had presumed to disturb the orthodox scientific teaching of this enlightened age. It is undoubtedly the fault of my book that it apparently comes into conflict with many ideas which have gained a place in our scientific text-books; but I believe that I am entitled to plead, in mitigation of my offence, that the various departments of science are often greatly in conflict with each other. In his "Physiology of Common Life," the late G. H. Lewes tells us that "we must never attempt to solve the problems of one science by the order of conceptions peculiar to another." I believe that the observation is pregnant with truth, and yet we would not be justified in concluding that there is some actual discordance in Nature which thus results in conclusions which are inharmonious, if not actually antagonistic. Rather we must infer that the seeming incongruity arises from the relativity of human knowledge, which is therefore in every direction imperfect. If I understand aright the view of creation which I have attempted to unfold in my book, instead of being in actual conflict with the great truths which are taught in our text-books, it goes far to reconcile many statements in the different departments of science which are more or less inconsistent with each other; and my faith in it is all the more because, as I believe, it also reconciles the fundamental doctrines of Religion with the great truths of Science.

My book might, appropriately enough, have been entitled "The Evolution of the Earth," but, as explained in the introductory chapter, its present title was chosen thirty-six years ago, when I published "An Essay on the Constitution

of the Earth," which contained, in a more or less crude and imperfect shape, the ideas which it is the object of the later work to elucidate, and when the term "Evolution" was not so scientifically popular as it is at the present time. Instead of being the result of some occult or mechanical disposition of things to change, I maintain that the evolution of species has been the result of a great scheme of creation, by which the Earth itself has undergone a progressive development. It seems to me that what are usually described as the "Laws of Nature" are simply *man's recognition of the order, harmony, or consistency, with which the Creator operates in the Universe.* I deny the existence of "immutable elements" or of "eternal and imperishable" matter in any shape. Matter, as it is known to our consciousness through the senses, is continually coming into existence and passing away. Only in the abstract sense can it be said to be unchangeable, for the simple reason that matter devoid of every quality by which it is known to us is only an abstraction, and has no more sensible existence than any other metaphysical conception. We recognise the continual activity of Nature when we observe that all things grow older. Time is, in fact, the symbol or abstract representative of the boundless succession of created things which have existed in the past or may exist in the future.

The order and consistency of the action of God in creation may be formulated as follows:—

1. *The Law of Existence.*—Circumstances govern the creation of things, and therefore all things exist by virtue of their circumstances.
2. *The Law of Diversity.*—As no two things can occupy the same place in the universe, therefore no two things can be exactly alike.
3. *The Law of Organisation, Variation, or Development.*—The differences or similitudes between two things must be in proportion to the differences or similitudes in the circumstances under which they have come into existence or by which they are sustained.
4. *The Law of Motion.*—A change in the circumstances of things necessarily involves a corresponding change in the things themselves.

Except for the theories of "immutable elements" and "imperishable matter," no one would think of disputing that no form of matter of which we have any sensible knowledge can exist independent of the conditions of its existence. As Paley, in his Natural Theology, has declared—"The bodies of animals

hold, in their constitution and properties, a close and important relation to natures altogether external to their own —to inanimate substances, and to the specific qualities of these; *e.g.*, they hold a strict relation to the elements by which they are surrounded." And Chemistry is essentially the science which notes the circumstances under which the various forms of inorganic matter exist, or by which they are changed.

The Law of Diversity follows the Law of Existence as an inevitable consequence, and the third law is also a necessary consequence of the first and second. If circumstances govern the creation of things, it follows that the differences or similitudes between two things must be in proportion to the differences or similitudes in the circumstances under which they have come into existence or by which they are sustained, which comprehend all the circumstances which have contributed to their actual present state. Hence each individual existence, or form of matter, may be described as an embodiment of all the circumstances through which it has passed from the remotest beginning.

Nor does the Law of Motion less strictly follow from the first law. If we admit that all things exist by virtue of their surroundings, a change in the circumstances of things must necessarily involve a corresponding change in the things themselves. It is obvious, however, that the effect of existing circumstances must be governed by the actual creation of the circumstances which preceded. Hence what I have described as Individuality comes into conflict with the existing conditions, and motion of some kind (chemical, physiological, or mechanical ?) is the result.

The belief that creation is a thing only of the past is utterly inconsistent with the religious idea of an ever-present and sustaining God. Evolution is, in fact, only another name for creation by the orderly and consistent action of an Almighty Creator. The same laws which have been observed in the evolution of species have been concerned in the origin and development of the globe upon which they are found. If plants and animals exist by virtue of the earth's existence, it follows that *they must be constituted according to the constitution of the earth.* The earth is constitutionally related to the sun; the condition of the air, the land, and the water, of animals and plants, is dependent on the earth's relationship to that body; the earth's relationship to, or dependency on, the sun is paramount to all relationship in respect of other bodies. If we assume that the earth has derived its existence from the sun, and been con-

stituted under its influence, we are presented with a reason of relationship as clear as that every effect must be consistent with its cause.

Thus, by an inductive process of reasoning, we find that the earth is an emanation from the sun. All the undoubted truths of Science are consistent with the conclusion that the Earth and all the other planets of our system have been derived from the sun by a natural process of progressive development; they are, in fact, the children of the Sun, born in the order in which we find them placed in our planetary system, the most remote being the oldest, and intervals of millions of years having transpired between the times of their first foundation. Thus there is a relation between the sizes of the planets and their distances from the sun; between the number of satellites of a planet and its distance from the sun; between the revolutionary motion of a planet and its distance from the sun; and between the rotary motion of a planet and its distance from the sun. Such relationship suggests a higher development in the planets which are most distant. The more distant planets are usually larger, have slower revolutionary and quicker rotary motions, and have more satellites, though such characteristics do not follow with that mathematical accuracy which we might expect if the relationship was of a purely mechanical instead of a physiological nature, as now propounded. The Moon, which I take to be the child of the Earth, has no rotary motion in relation to its primary; it has no atmosphere; it is destitute of water; and no signs have yet been discovered of vegetation or inhabitants. Born, as I assume, long ages after the Earth, it is altogether in an inferior state of development.

Geology admittedly points to a time when there was neither animal nor vegetable life on the earth. It is supposed to be an established fact that organic life originated in the simplest forms, which were slowly followed by others of a higher type; and all the changes which have taken place are believed to be due to the present "order of Nature."

Such conclusions are inconsistent with the idea of an accidental mechanical arrangement of the stratified rocks which constitute the outward covering of our globe, and which, for the most part, present the appearance of aggregations of matter consequent upon chemical or physiological processes. Moreover, they suggest the idea of being actual *additions* to the original matter of which the earth was formed before organic life began to be developed. That they

are, in the case of coal and lime, at least temporary additions is beyond dispute. The coral zoophyte has the power of secreting lime from the waters of the ocean, with which it builds up reefs of vast thickness, and entire islands are thus created. As the result of the *Challenger* Expedition, it has been discovered that the minute shells of microscopic animals which live at or near the surface are constantly being showered down upon the bottom of the ocean, where they are ultimately transformed into limestone and ironstone. An estimate of the enormous extent of this deposit may be formed by reference to the fact that the ocean covers three-fourths of the entire globe. Upon the mechanical theory of the formation of the earth, it is assumed that the lime thus deposited was previously held in suspension in the water; but the chemist informs us that salt, not lime, is the chief ingredient of sea-water. This deposit of lime has been going on for millions of ages, and there is no reason to believe that the rate of increase is at all diminishing. Moreover, Sir Wyville Thomson concludes that the "'red clay' is not an additional substance introduced from without," but "is produced by the removal, by some means or other, over these [deep] areas of the carbonate of lime, which forms probably about 98 per cent of the material of the 'globigerina-ooze.'"

The natural inference is that water, like every other form of matter of which we have any *real* (as contrasted with theoretical) knowledge, is undergoing a constant change, by which lime, iron, salt, and numerous other substances, are constantly being developed or evolved; such evolution being the result of general or special changes of circumstances to which water is continually being subjected. The theory of the mechanical circulation of water, instead of being founded upon facts, is largely a creation of the imagination. In place of being immutable or unchangeable, water is one of the most unstable substances of which we have any knowledge. Under mere changes of temperature, it (that is, the abstract article) is alternately a vapour, a liquid, or a solid; under some conditions it becomes putrid, whilst under others it forms the chief substance of the growing plant or the living animal. In the human body it is an important element in the various processes by which upwards of fifty more or less stable substances are formed. We have, indeed, no reason to believe that water is an exception to the other various forms of matter of which, through the senses, we have any conscious knowledge. *As a fact*, it falls upon the earth to a depth which has been estimated at

from 20 to as much as 250 inches in a year; and, if we take the average fall at 36 inches, the ponderable matter which thus falls upon every square mile of surface amounts, in round numbers, to no less than 2,326,000 tons. That, in certain dry and warm conditions of the air, some portions of this water rise again in the form of vapour is undoubtedly true, but even such comparatively small quantity probably all falls once more in the form of dew. Certainly it is a *pure theory* that all the water which falls is lifted up again to produce those beautiful creations of the atmosphere which, beginning in the light and fleecy curl-cloud, pass, by a process of growth, to the dark grey rain-cloud, and finally fall in rain, snow, or hail.

True, we cannot take pure water, which is said to be a compound of oxygen and hydrogen, and transform it at pleasure into salt, or lime, or iron. Man can only work successfully in the ways of God. We cannot transform, by any mere effort of our will, the acorn into the oak tree, and yet, if the acorn is planted in the soil and subjected to all the other necessary conditions, it will grow into an oak tree. Man has discovered the means by which a mixture of two light gases can be transformed into water,—why should water not be changeable into salt? We certainly know how to reverse the process. By means of heat, salt can be fused or liquefied, and so transformed into vapour or gas, in which condition it will return to the air from whence it descended, modified, however, by the circumstances through which it has passed.

Nor is this a view of creation essentially at variance with the accepted theory of scientists as to the formation of the earth. The theory of Immanuel Kant assumes that every form of matter was created out of gas, or the ethereal substance which occupied boundless space. I assume the same, with this difference, that, whilst Kant propounded an entirely fanciful process, I maintain that all the elementary bodies, and every form of matter of which the senses can take cognizance, have been the result of natural processes which are now going on. He set up an idol in the name of gravity; I worship only the "living and true God," whose power and presence I recognise in all created things.

If the earth be not an emanation from the sun by means of the laws of God in creation to which I have briefly referred in this article, but which are described more at length in my book, how have all the several kinds of "immutable atoms" of which the elementary bodies are composed, and which it is assumed once existed in the form of gas, been

collected together and formed into the solid substances which now exist. The theory of gravitation, as now taught, returns no answer to this question; but if, as Newton suggested, the cause of gravity is to be found "in the general laws of Nature by which the things themselves are formed," the solution is consistent with the constitution of the earth as now expounded.

The space at my disposal prevents me from entering at length into the physical evidences of the earth's expansion. These, however, exist in abundance in the cracks, veins, fissures, ravines, and valleys which are to be found in all directions on the earth's surface, and which present in a large way very much the appearance of the cracks in the bark of growing trees. Assume that the crust of the earth is an addition to its surface, and that, besides the mechanical and other movements described by geologists, we have to add the important effects of growth, and all the difficulties and disputes which prevail, as to the meaning of existing phenomena, are satisfactorily solved.

The sun is often described as the source of life, and its importance in connexion with all the physiological processes which take place on the earth demonstrate the existence of a relationship the reverse of mechanical. It is the active cause of light and heat, and the fossil sunbeams of millions of ages ago are now used to light up our factories and workshops when, by the rotation of the earth on its axis, we cease to enjoy the direct effect of its life-creating influence. Even the colours of the ancient sunshine have not been lost to us, but are reproduced and utilised in the shape of aniline dyes. The spectroscope, as I have endeavoured to demonstrate in my book, not only decomposes the light of the sun into all the colours of the rainbow, but also exhibits evidence of the presence of numerous embryonic elements, which may be capable, under the necessary conditions, of being converted into the solid state. Nor is the proof by measurement of the earth's growth absent, Capt. Drayson having shown that, whenever a base line has been measured a second time, after the absence of years, the later measurement was invariably found to be longer than the former. And the latest measurement of the earth's distance from the sun is also confirmatory of the view of creation now propounded; the most careful calculations of eminent astronomers, in connexion with the transit of Venus, showing that the distance between these two bodies is increasing, former measurements having also supported the same conclusion.

## III. THE PRESENT STATE OF THE VIVISECTION QUESTION.*

WHEN the Anti-Vivisection outcry had become so far successful that a Royal Commission was appointed to investigate the charges brought against medical men and experimental physiologists, and when a Bill for official interference was introduced under the auspices of the late Ministry, a professional contemporary expressed the amiable hope that the measure would not "in any material way" hinder the progress of research, and would "calm the needless apprehension and put an end to the odious misrepresentations which have been recently rife concerning this subject, and which have been in ignorance adopted by persons of consideration, who will probably in future take more pains to be correctly informed." We ventured at the time to express our fears that the result would be very different.† Our misgivings have been more than verified. Research has been impeded to a very serious degree. In proof we need only quote the instance narrated by Dr. Pye-Smith, F.R.S., in an Address delivered at the Sheffield Meeting of the British Association. It appears that "two thoroughly qualified men were anxious to carry out an important investigation on the treatment of snake-bites. They procured venomous snakes from a distance, and applied for the special certificates necessary. Considerable delay ensued; various objections were raised and set at rest, and at last all the certificates were obtained; but meantime the snakes had died!" Dr. Ogle‡ informs us that "at a certain hospital experiments on a most important investigation had not been carried out, owing to the risk of bringing that hospital into public notoriety." Thus between the law and Mrs. Grundy, research is interfered with in a most material way.

Yet, although biological investigations requiring experiments upon living animals are now rendered all but impossible, it must not be thought that the agitation has decreased,

* Ueber den Wissenschaftlichen Missbrauch der Vivisection. Von FRIEDRICH ZÖLLNER, Professor der Astrophysik an der Universität Leipzig. Leipzig: L. Staackman.

† Quarterly Journal of Science, vi. (1876), p. 333.

‡ Harveian Oration for 1880, reported in Medical Press and Circular, June 30, 1880.

that the enormous concessions made are accepted as final, and that the odious misrepresentations have been withdrawn. According to the Report for 1878 of the Inspector under the Vivisection Act, only ten licenses have been applied for in Ireland; only five of these have been acted on, and "no pain has been inflicted even in one of the twenty-four experiments performed." For all this the "Irish Anti-Vivisection Society" still exists and continues its labours! Indeed it seems to us that since the passing of the Act 39 and 40 Vict. the agitation has become even more unscrupulous in its character. We know that the most questionable means have been used to obtain signatures to the petitions for the abolition of vivisection. Persons, children included, who have never given the subject a moment's thought, and who scarcely know what "vivisection" means, are besieged to "just sign their names," and for the sake of quietness they often consent. We know a young lady, the daughter of an eminent artist, who, whilst copying a picture in one of our public art-galleries, was accosted by two strange ladies, who pressed very hard for her signature, and when she declined, on the rational ground that she was totally ignorant of the matter, went off quite exasperated. Is it likely that she was the only young student thus solicited? Perhaps if Prof. Zöllner were aware of the manner in which agitations are got up and conducted in England, he would lay much less weight on the number of signatures to the petitions against vivisection.

At one time it was contended by the anti-vivisectionists that Harvey's great discovery was not due to experiments upon living animals. Finding that position untenable, they now seek to throw discredit upon Harvey, and insinuate that he had been anticipated by Servetus. We have seen this charge against our great English physiologist stated in an advertisement which, if not carefully read, may lead to the belief that Sir W. W. Gull, M.D., F.R.S., and other heads of the profession, are connected with the Anti-vivisection agitation.

It must be distinctly understood, however, that the rival societies for the suppression of research—bodies not too amicable among themselves—have not succeeded in bringing forward any cogent or novel arguments on their side, or in meeting those advanced by the defenders of research. The great charge of inconsistency brought against the agitators* has never been met. A peer of the realm did, indeed,

* *See* Quarterly Journal of Science, vi. (1876), p. 318.

indulge in the hacknied sophism that "one wrong does not excuse another." We bid him first prove that the infliction of pain and death upon animals for important purposes is a wrong at all. A certain literary organ tells us that if we can only learn how to escape diseases at the cost of the lower animals, then *noblesse oblige* that we should be content to suffer! It might be thought that even the editor of a "Society journal" could scarcely fail to see that the same principle, if it holds good here, must *à fortiori* forbid the infliction of pain and death upon animals for amusement, for convenience, or for luxury.

There is another inconsistency in the position. As the law at present stands we may, if we think proper, keep a number of live cobras, rattlesnakes, &c. We may feed them with rats, mice, guinea-pigs, rabbits, and we may even take pleasure, if so disposed, in the sufferings of the victims. In all this we neither break the law nor are our proceedings denounced as "orgies of diabolism." But suppose, after a rabbit has been bitten, we take him out and try to cure him, we are then performing a painful and cruel experiment. Unless we have a license from the Home Secretary we are in peril of fine and imprisonment, and whether we hold such certificate or not we shall receive anonymous post-cards fraught with abuse. Nay, if we even take the wounded rabbit, and after his death submit his fluids and tissues to chemical or microscopical examination, we are probably liable to the same doom. Or, again, we may poison rats and mice, if we think proper, with strychnine, phosphorus, arsenic, or baryta, without fear of legal consequences. If we have a dog or a cat which we wish to get rid of, no one questions our right to give it a dose of potassium cyanide. In short, we are allowed to poison animals at pleasure so long as they are our own property. But suppose that if we mix with the food of such rats, dogs, or cats, some unknown drug or some novel chemical preparation, and mark the results; or suppose we administer to such creatures an extract obtained from the viscera of an animal or man supposed to have been poisoned? We have then broken the law, and outraged public opinion. Thus we see that actions which are innocent as long as performed from wantonness, from the sheer love of killing, or at best to get rid of a nuisance, become criminal if done for the solution of an important physiological question: the higher the motive, the worse the action! This, then, is the outcome of humanitarian meddling and muddling and of governmental submission to the demands of organised ignorance.

We are not aware that our British anti-vivisectionists have ever attempted to justify the contradictions and inconsistencies which have just been pointed out. But as the agitation has become "international,"—or epidemic, which is much the same thing,—we felt bound to hear what foreign denouncers of biological experimentation might have to advance. One work especially attracted our attention. Its author, Prof. Zöllner, is a man of totally different calibre from the English anti-vivisectionist oracles. He is not a hysterical novelist nor a sensational philosopher. He is neither a sportsman quite ready to practise and uphold killing for killing's sake, nor a worshipper of Anubis, prepared to inflict any inconvenience or injury upon his fellowmen so that dogs may be made more comfortable. Nor, lastly, is he a physician or surgeon, careless for the advance of Science so long as his own practice may be increased. On the contrary, he is a thinker whose erudition is no less wonderful than his acuteness and profundity, who has "won his spurs" in physical and astronomical research, and whose passionate love for truth and fearlessness in its utterance are universally recognised.

It might naturally be awaited that such a man, if he entered upon this question at all, would do it fundamental justice. We expected a philosophic discussion of the nature and limits of man's sovereignty over the lower animals, and of his rights to inflict upon them pain or death. We trusted that he would have shown whether, in how far, and under what circumstances vivisection is in excess of such rights, and a departure from man's duties towards his fellow tenants of the globe. We thought he would show, or at least attempt to show, when the conclusions drawn from vivisectory experiments were trustworthy, and when they might be regarded as illusive. This we felt the more inclined to expect since he states that "the right of judgment on the scientific-physiological value of vivisection in its previous extent belongs in the first place to physicists, and not to physiologists and physicians." This is a paradox which we are not prepared to accept. We conceived it quite possible that we might meet in this book arguments against experimentation such as we have never before encountered, and which might seriously alter our convictions.

We must first note that Prof. Zöllner is not an advocate of the "total abolition and utter suppression" of vivisection. He denounces its abuse (*missbrauch*). Now it would be utterly illogical to speak of the abuse of a "grievous sin"*

* See bills displayed at railway-stations and elsewhere.

of an evil which it is sought utterly to abolish. The abuse of anything implies that there is a legitimate use. Hence Prof. Zöllner is by no means to be regarded as one of those advocates, blind to facts and deaf to reason, who seem to spring up in connection with every agitation and movement. Yet we scarcely see that he draws the line between the use and the abuse of vivisection with the clearness which would be desirable, and his language at times seems to savour of the most violent English school. Still, taking the title of the book into consideration, we are bound to put the most favourable construction upon his words. On our part we are free to admit that vivisection has been abused, and that some writers—not content with defending its legitimate and necessary employment—have weakened their position by denying or justifying such abuses. We fully grant that painful experiments upon living animals should be undertaken simply for purposes of research, for the solution of novel or undecided questions. We would neither practise nor countenance vivisection for mere demonstration, or for the acquirement or display of manipulative skill. We hold that every animal irremediably injured should be at once put to death after the operation or experiment is at an end. We deplore that physiologists should in any degree have departed from these conditions, and should thus have given a foothold for agitators and sentimentalists, and have gravely compromised the future of biological research. But whilst admitting and regretting such aberrations and excesses, we must not forget that nothing of the sort is chargeable to English men of Science in modern days. Even the Secretary of the Society for the Prevention of Cruelty to Animals, when giving evidence before the Royal Commission on Vivisection, acknowledged that he did not know of a single case of wanton cruelty.* Such being the facts, and due regard being had to the ultra-humanitarian tendencies of the nation which—save when sport or fashion is concerned—is so exceedingly averse to give pain, it might have been thought that special legislation was unnecessary, and that public opinion would have proved amply sufficient to restrain the abuses of vivisection.

But whilst finding in the fundamental idea of Professor Zöllner's work much that we can accept, we regret to note a striking absence of those features which we considered ourselves warranted to expect. The vivisection question is

* It would have been well had Prof. Zöllner and others noticed how much the Report of the Royal Commission goes beyond the facts elicited, and how far it is, in turn, outstripped by the Bill as proposed, and even as carried.

here so much mixed up with other matter that were the title, the table of contents, and the heads of chapters struck out, few persons, if any, would consider the work as an anti-vivisectionist treatise. The author's attention is continually drawn off from his ostensible subject to the influence of the Jews in Germany, to their conduct in the Reichstag, in the Universities (where they appear to hold a number of professorships very high in proportion to the percentage which they form of the German nation), and in the political and literary press. It appears that there are in the German universities no fewer than seventy professors of unmixed Hebrew race—more than three times the number which might be expected from the proportion of Jews in the empire. Concerning this fact Prof. Zöllner, like many Germans in all ranks of life, appears greatly exercised. To this subject he returns again and again, and no small portion of the work is taken up with remarks on Herr Lasker, a Jewish member of the German parliament, and of Lasalle, also a Jew, and the founder of social democracy in Germany. The space devoted to these two men seems—in view of their very remote and doubtful connection with the subject of the work—surprising. Social democracy and Russian nihilism are also introduced, apparently in virtue of the circumstance that the would-be regicide, Nobiling, had enjoyed a medical education, and had possibly been a spectator of vivisections.

Prof. Zöllner is no admirer of the modern mania for the "higher education" of women and for the admission of female students to the medical schools. He relates, with justifiable indignation, that at the University of Geneva two young ladies were busy with the dissection of the lower extremities of a male subject, whilst two young men were at the same time engaged on the head and breast. There is also a notice of the scandalous career of the female students of the University of St. Petersburg, which we do not care to quote. The author returns to the subject in a subsequent portion of his work, and grows somewhat Rabelaisian thereon. We fully share his displeasure, but cannot reproduce his remarks, humorous though they be.

We find, also, certain interesting statistical results concerning the kingdom of Saxony, the region where it appears that the anti-vivisectionist agitation is more rampant than in any other part of Germany. It appears that the total yearly number of condemned criminals has increased from 11,001 in 1871 to 21,319 in 1877, or a total increase of 93·79 per cent, as against an increase in population of 7 per cent. Murders and other crimes of violence have multiplied

at the rate of 473 per cent, and indecent assaults upon children under 14 years of age in the proportion of 825 per cent. The author refuses to attribute this moral decline to the consequences of the war, but ascribes it to the teachings of the schools, the universities, and of the press.

Another subject which plays here no small part is Spiritualism. By the prosecution of Slade we are told that England "set the seal on its intellectual decline." On this subject we wish to give no uncertain sound: much as we honour Prof. E. Ray Lankester for his achievements in biological science, we hold that in the Slade prosecution he committed a fearful mistake. To hand over to solicitors and counsel, to police magistrates and quarter sessions, a question which, if capable of solution at all, can only be decided by men of Science, was a piece of renunciation or self-abnegation which cannot be too deeply deplored, and which is doubly to be regretted in a country where Science is so little honoured as in England. However much Spiritualism may have been complicated by deceptions or delusions, it is the duty of scientific men to *make sure* that there is in the phenomena produced nothing more than is fairly referrible to jugglery or "unconscious cerebration." Till this has been done, to call in the aid of such rough and ready tests of truth as courts of justice can supply is nothing short of a formal abdication and a confession of impotence. At the same time, had Prof. Zöllner been better acquainted with our institutions he would scarcely have found, in this prosecution, grounds of accusation against either Government or people. He is doubtless not aware that so long as a statute—however obsolete—remains unrepealed the authorities cannot prevent any person from applying it to his purposes. In connection with this subject it must not be forgotten that Prof. Zöllner, unlike some of his English colleagues, proposes, or at least hints at, a substitute for vivisection,—to wit, "bio-magnetism." This idea surely had better have been developed in full, even if some of the personalities here introduced had been omitted. Such personalities, indeed, form a most strange—and we do not hesitate to say a most deplorable—feature in Prof. Zöllner's work. Virchow, Helmholtz, Du Bois-Reymond, Vogt, Pagenstecher, Bernard,—every physiologist of note who has either practised or defended vivisection,—is denounced in a manner which is very far from carrying conviction to the mind of the reader. Let us take a specimen: Professor Tyndall is no biologist, and has probably never dissected any animal, living or dead; but he publicly declared, in 1875,

that he should regard any interference with experimental* vivisection as a great misfortune to Science. He is therefore treated to four pages of irrelevant personality. Our author writes: "Prof. Tyndall decided a few years ago, at the age of 57, to enter into the holy estate of matrimony. The sacrifice (*opfer*) of his ardent affection was a Lady Hamilton, a wealthy lady belonging to the highest English aristocracy. I cannot say with certainty whether the happy or unhappy spouse of Tyndall belongs to that eccentric family of Hamiltons from which the hereditary Prince Albert of Monaco selected a wife, from whom he is now being divorced." The Monaco scandal is then given in detail in a note, and the author proceeds to quote a letter which Prof. Tyndall is said to have written during his courtship, as a model for "Darwinians and Materialists." We must here ask—Is this scientific criticism? Is it in any way relevant to vivisection? Is it worthy of an author of Prof. Zöllner's merit? This style of controversy is the more to be regretted since he appears highly indignant at any personalities on the part of his opponents. What, again, shall we say of the following passage?—"At that time Berlin, the learned and cultivated, was so completely chloroformised by the *fœtor Judaicus* that it listened as devoutly to the Hebrew melodies of the lyric poet Heine, and to the oracles of the naturalist Alexander von Humboldt, as it now does to the songs of Rudolf Löwenstein and the theories of the academicians Virchow and E. du Bois-Reymond." Humboldt died before the vivisection question broke out, but he is repeatedly made here the object of unfriendly remarks. Prof. Zöllner seems to have many enemies, living and dead, and he may have the best causes for his enmity. But what light do all these disputes and grudges throw upon the subject in hand? Do they prove vivisection wrong? Do they mark out the limits, if any, within which it ought to be tolerated? An interesting and instructive fact here mentioned might well be taken to heart by English biologists, in view of the restrictions under which they already labour and of the further attacks with which they are menaced. The medical faculties of the following universities have formally protested against the anti-vivisection agitation:—Bâle, Bern, Bonn, Dorpat, Erlangen, Freiburg, Graetz, Greifswalde, Halle, Heidelberg, Kiel, Königsberg, Leipzig, Marburg, Munich, Prague, Vienna, and Zürich. Could no similar expression of qualified opinion be produced in the United Kingdom?

* We call attention to the word "experimental." Prof. Tyndall does not appear to have advocated vivisection for mere demonstration.

The main points of Prof. Zöllner's work, stripped from all irrelevancies, are—First, his approval of restrictions and limitations on vivisection on behalf of Government. Such regulations, we admit, might work much better in Germany, where the authorities respect Science more than in England and interfere less clumsily. If licenses to perform painful experiments are required in any continental country, they will be issued by some competent and qualified official, and not by a Secretary of State whose time is engrossed with party affairs, and who knows and cares as much about biology as does an average bricklayer. Secondly, the proposal of a substitute for vivisection, most welcome if practicable, but on which the learned author might have been more explicit. Thirdly, the claim that physicists are better judges than physiologists of the value and necessity of vivisection,—a doctrine which we shall be prepared to accept when it is proved that organisms are mere machines or automata, when the chasm between animation and inanimation has been bridged over, and when irritability is shown to be merely a special case of attraction and repulsion. But, if we mistake not, Prof. Zöllner is as far removed as ourselves from believing in the probability of such a consummation.

# ON WATER AND AIR.*

By JOHN TYNDALL, D.C.L., LL.D., F.R.S.,
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## LECTURE VI.

IN our last lecture we entered upon the examination of air, and I confined myself in that examination to what we call the physical properties of air, not going into its chemical qualities. We learnt how the pressure of the atmosphere was determined,—how that immortal experiment of Torricelli was made; and, as I said at the time, I think that no more impressive experiment ever could have been made, or at least that there was no greater example of that outgoing, so to say, of the human mind in advance of experiment, than in the case of Torricelli. It was an instance of pure reasoning by which he inferred that a mercury column 30 inches high would be supported by the pressure of the atmosphere. His seeing this beforehand, and his bringing his foresight to the test of fact, and seeing that fact realised and experienced, must have given him a joy and pleasure not frequently experienced.

Well, this was in 1654; but immediately afterwards the discovery of the air-pump placed in the hands of investigators a means of pursuing this subject farther, and one great man—a man whose name is for ever memorable—threw himself with ardour at the time into the investigation of this subject. This was the illustrious Robert Boyle. He improved the air-pump, and he made a great number of experiments with his improved "pneumatic machine," as it was called sometimes. He was a man of great thought and great power of experiment, and great reasoning power. Herein consists the power of a philosopher in examining Nature—that he shall have power not only to investigate the facts of Nature by inventing experiments, but the power of reasoning out those experiments to their legitimate results. Boyle possessed both these qualities in a high

* Being a Course of Six Lectures adapted to a Juvenile Auditory, delivered at the Royal Institution of Great Britain, Christmas, 1879. Specially reported for "The Journal of Science."

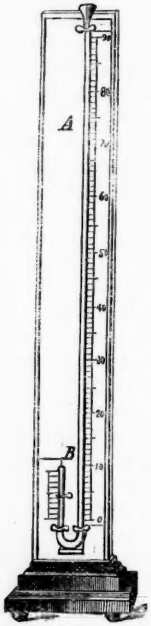
degree, and it was soon found, after the experiment of Torricelli, that the height of the column of mercury varied from day to day, and varied sometimes very rapidly indeed; and that proved to people in those days that the pressure of the atmosphere was by no means a constant thing. Sometimes the atmosphere was lighter, and sometimes it was heavier than at other times; and Boyle observed for five weeks the oscillations of the height of this barometric column, and in that way saw clearly the variations in the pressure of the atmosphere. It is these pressures, or at least these variations of pressure, that now enable us more or less to predict the advent of certain kinds of weather. Here in London, for instance, when the north-east wind blows, you would usually find a high barometer as it is called,—that is to say, the atmosphere, being chilled by the north-east wind, is denser or heavier, and it is able to support a higher mercurial column than when, for instance, the west wind blows, which comes to us charged with light aqueous vapour from the Atlantic, warmed more or less, and the consequence is that this lighter air is not able to sustain the same height of the mercurial column. Well, this was one of the early observations made by Robert Boyle, and he made various other observations with his air-pump. He made observations upon the influence of air upon respiration, and we know a great deal about these matters now; but it was of infinite importance to break ground in this region—to get some experimental grasp of this region which was not at all known before. The proper action of the air upon animals was not known, and he put snails and caterpillars, and mice and rats, and I think even a dog, under the receiver of his air-pump, to observe the effect of withdrawing the air from those animals. He produced very wonderful results, which caused him to lift his heart in gratitude to the Creator for making the air so essential to animal life and enjoyment.

I dwell upon Boyle particularly, because he was a scientific character and a philosopher worthy of all imitation by the younger scientific philosopher. He was a man who not only possessed the highest intellectual culture of his age, and who made important discoveries, but he united to this power of a scientific man those characters and qualities that we essentially ascribe to the human heart—the qualities of tenderness, and courage, and courtesy. And let me say to you, my young friends, once for all, that scientific knowledge unless backed up by those other qualities leaves you not rounded men, but only lopsided men.

Well, this I have thought it my duty to say with regard

to the illustrious Robert Boyle. One experiment which has been made has an immortal interest, and is of the utmost importance in relation to this subject. I have here (Fig. 37) a simple instrument, similar to the one with which Boyle operated. The long arm (A) of the bent tube (A B) is open at the top. The short arm is closed at B. We have introduced some mercury into the bent part of the tube, and the fluid metal now stands at the same level (at o) in both the

FIG. 37.




long and short arms. The space above the mercury in the closed tube contains a certain bulk of atmospheric air, submitted to the ordinary pressure of the air through the open tube A. And now I will ask Mr. Cottrell to pour carefully into this longer arm a quantity of mercury. What will occur? The mercury falling into the tube will press the mercury which is in the bent part up into the shorter arm. The air in this short arm will be squeezed more forcibly together, and the greater the amount of mercury which we

pour into this longer tube the greater will be the amount of squeezing. Now, what Boyle wanted to ascertain is this: what is the relation between the pressure of the mercury brought to bear upon this air and the volume of the air; and I will tell you what his result was. He found that when the pressure was doubled the volume was halved, and when the pressure was quadrupled the volume was reduced to one-quarter, so that as the pressure augmented the volume decreased in precisely the same proportion. Or, turning it into other language, instead of "volume" let me say "density." The density means the quantity of matter crowded into a certain space. If we use the term density Boyle's law would be this, that the density of the air is directly proportional to the pressure exerted upon it.

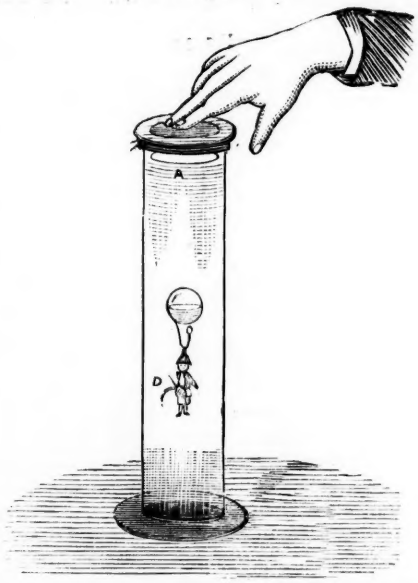
And now the mercury has risen in the long tube, and has reduced the volume of the air enclosed in the short arm to one-half its previous volume. If we now measure the height of the two columns, we shall find that the level of the mercury in the long arm stands very nearly at 30 inches above that in the closed portion. Thus the pressure of an additional atmosphere has reduced the bulk of the contained air to one-half. In this way, then, by doubling the pressure we halve the volume of the air, and by quadrupling it we should render the volume of the air one-fourth. In this way Boyle proved that the density of the air was exactly proportional to the pressure brought to bear upon it. I could not possibly dwell upon the subject were it not that I think that honour ought to be given where it is due; and for a long series of generations, I may say, the discovery of this law has been ascribed to another person,—a most meritorious man,—a philosopher named Mariotte; and you will find that in books, particularly in Continental works, this discovery is always referred to as the law of Mariotte, but Boyle preceded Mariotte. He took the greatest care in his experiments, and he established with the utmost rigour the existence of this law anterior to Mariotte, and therefore the law ought undoubtedly to be called the law of Boyle instead of the law of Mariotte. Boyle, as I mentioned in our last lecture, had very clear notions regarding pressure of the atmosphere, and regarding what he described in beautiful, or, I may say, almost poetic language; for men like Boyle have a strong poetic instinct,—that is to say, they can see resemblances between utterly remote things. They can pierce, as it were, into the nature of things, and see things resembling each other where the common eye sees no resemblance whatever; and thus, as I expressed in my last

lecture, he compared the particles of air to little springs pressing against each other. When the air was pressed these springs yielded, and the air was forced into a smaller space. When the pressure was relaxed these little spiral springs, as it were, expanded, and caused the air to expand. In this way he figured the mechanical action of the air, and he wrote some celebrated papers on what he called the weight and the spring of the air. There are one or two illustrations of this spring of air which I think will impress it upon your mind. One which I should like to mention to you occurs to me just at the moment. Sometimes in going up a mountain you notice a curious pain in the ear; sometimes, at intervals, you feel as if a kind of bubble of gas got into your ear: some persons are subject to this feeling, and some are not. The cause of this effect is this:—Here we have the passage of the ear, through which all sound, or the great body of sound, passes. That passage is stopped at a certain place by a membrane called the tympanic membrane, or, to use the common term, the drum of the ear. Beyond that there is another membrane. When the sound vibrations come into the ear they strike upon the drum of the ear, and the vibrations are transmitted to the membrane on the other side, and sent on to the auditory nerve, and from the auditory nerve to the brain, where they are translated into sound. It is necessary that the same pressure should exist within the drum of the ear and without it. If the pressure were greater within than without it would bulge the tympanic membrane or drum-head outwards, and cause pain. If the pressure within were less than without, the excess of the pressure of the atmosphere outside would force the drum inwards, and cause pain; and this is very frequently the case. I do not experience the pain myself, but I have often walked beside others who have experienced it. How is this pain to be avoided? There is, among the wonderful adaptations that we see here, a tube from the mouth to the drum of the ear: that is the only passage to the ear. This tube is called the Eustachian tube. When you are climbing a mountain, and feel this inconvenience, simply go through the process of swallowing. The act of swallowing opens the Eustachian tube, and allows the air to get in or to get out, as the case may be, and so establishes an equilibrium between the air within and the air without, and thus you have all the inconvenience removed. This is due to what Boyle called the spring of the air.

There is another instance of the same spring which I dare say you are all well acquainted with. It is called the

Cartesian Diver. The diver (Fig. 38) consists of a glass figure having attached to its head a hollow ball. The figure and ball are entirely closed, except a small opening in the curled tail, D, which is affixed to the body of the figure. A quantity of water has been introduced into the figure and ball, so that at the present time it is slightly lighter than the water in which it is contained. In the jar, A B, the proportions between the air and the water are such that the diver floats; but if I press upon this sheet india-rubber

FIG. 38.

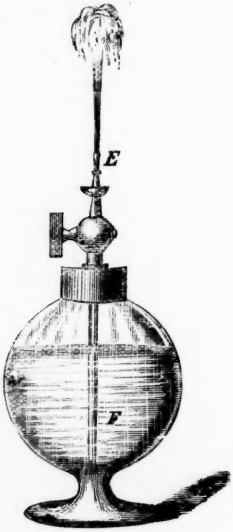



with which the top of the jar is closed, what do I do? I squeeze the air at the top of the water, there being a little air underneath this piece of india-rubber. The compression of the air is transmitted to the water; and this is the point that I want to make clear to you. The water has the power of transmitting the pressure from the air above it, and that pressure is transmitted to the little diver below. What is the effect? It is that the pressure squeezes a certain amount of water into the diver, and the consequence is that the air within the hollow ball is compressed, and is replaced

to a certain extent with water; and this makes the little figure heavier, and down it goes; it sinks to the bottom of the jar. I now release the pressure, and the spring of the air within the ball ejects the water that was forced into it, and the figure rises to the surface.

Now let us make another experiment. I will press this covering, and at a certain point I will suddenly withdraw the pressure. What will occur? Boyle's spring of the air will again come into play. The air that is compressed in the hollow ball will suddenly relax itself, will suddenly expand like a compressed spring, and will drive out the water

FIG. 39.



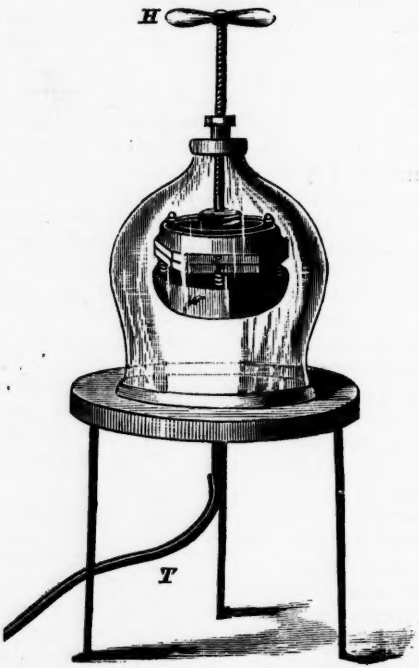

in the tail as before, and, inasmuch as the tail is curled round, the spring of the air will cause the little creature to pirouette. The spring of the air might be illustrated in fifty ways. For instance, here (Fig. 39) I have a small fountain. It is a spherical vessel, which has inserted into it a metal tube, E F, provided with a stopcock. It is about two-thirds filled with water. I have here a condensing syringe, which I will attach to the end of the tube at E, and by means of it will compress air into the fountain. The air will pass down the tube E F, and collect in the space

above the water. Now, having forced sufficient into the fountain, I will shut the stopcock and remove the syringe. Many fountains play upon this principle, but it is simply an illustration of the spring of air which was so completely investigated by Robert Boyle. At the present time you have compression of the air by means of this spring to which Robert Boyle refers. This spring of the air which has been forced upon the surface of the water will drive out the water. I will now open the stopcock, and you see that the water is ejected through the tube E F to a considerable height, and continues to play until the vessel is emptied.

There is another experiment referred to by Boyle to which I will draw your attention. Here are two plates of marble that have been in this institution for fifty or sixty years; at all events they were here long before my arrival, and they were obviously made for the purpose of illustrating the experiment of Boyle. We take these two plates of marble, which are intended to be very smooth, and slide their flat surfaces over each other: there is a certain definite way of causing them to cling together. I feel the clinging force very much. They ought to cling and hold fast together. Now Boyle entertained the idea—and the idea has come down to the present time—that when you pass one of these plates over the other, and cause them to come into close contact with each other, you squeeze out the air, and hence the atmospheric pressure outside comes into play and keeps the plates of marble together. I am indebted to one of the greatest mechanical geniuses of our age, Sir Joseph Whitworth, for the two metal planes that are now before you. I want to show you their action. These two planes of iron were produced by Sir Joseph Whitworth, who has done this in a manner that really is perfectly wonderful. He has produced what are called true planes. He started in life with the idea of making mechanism as perfect as possible, and his first idea was to obtain perfectly flat surfaces. I wish I had time to go fully into the manner in which he has done it. He has done it in the simplest manner possible, by first of all making them approximate to smooth surfaces. He then placed a coating of rouge on one of the surfaces, and, placing this upon the other surface, pressed them together. If they were perfectly smooth the rouge would cover both surfaces uniformly; but there are always little hollows, and hills, and eminences, which left the rouge in spots and patches, and by scraping away these eminences he arrives at his true planes. When I place the two together there is a most extraordinary effect. The upper one

appears to be floating upon a kind of oil, and that is due undoubtedly to the air beneath; and it was supposed, even by Whitworth himself, that when you press down this plate you by so doing squeezed out the air, and that this one was firmly clasped to the other, and that this firm clasping of the under plate to the upper plate was due to the air being squeezed out. There are many who thought that this was not the case, and that it was a case of firm molecular cohesion. To experimentally test this question the following

FIG. 40.




arrangement was made:—Here are two Whitworth planes, P, P (Fig. 40); they are placed under the receiver of an air-pump. The planes have been pressed together, and are firmly adhering. The under plane has attached to it a weight of lead weighing 20 lbs. The upper plane is attached to a rod which slides air-tight through the upper part of the receiver, and by means of the handle H I am able to lift the

planes with the lead weight attached. The receiver is placed air-tight upon a circular plate, and connected to an air-pump by the tube T. By means of the pump we have exhausted the receiver of air, and the lower plane, with its lead weight attached, is hanging there, adhering firmly to the upper plane. Had a weight of 100 lbs., instead of 20 lbs., been attached to the lower plane, it would have been sustained by the powerful attraction of the two surfaces, so that this clinging force, whatever it is, is independent of the pressure of the atmosphere. It is, in point of fact, part and parcel of the force which causes the individual molecules of the air itself to cling together. If you bring the two surfaces closely together the molecular cohesion comes into play, and holds the plates together. This experiment shows that in this particular, at all events, Boyle was not correct in his conclusions; but the number of things in which he was correct is simply wonderful. He observed, as I have said, those changes in the height of the mercurial barometer, and he observed many other things. Usually the credit of proving that sound cannot pass through a vacuum was ascribed to a most meritorious man, and far be it from me to say a word against him,—to Hawksbee, in 1709,—but nearly half a century previously Boyle actually put his watch under the receiver of his air-pump, and he found that when he exhausted the receiver the ticking of his watch ceased to be heard; so that nearly half a century before the time of Hawksbee, Boyle had proved this fact. This error on his part, in his conclusion as to the cause of the clinging of the plane surfaces, is a mere trivial thing compared with the amount of truth which he established.

Now I have done referring to the transmission of the pressure through the water towards this little Cartesian diver. I do not know whether the experiment which I am now going to make is destined to succeed, because I had not the opportunity of making the experiment beforehand, having only one of these bottles at hand; but I want to see whether the transmission of a shock through this water will be sufficient to produce a striking mechanical effect which will be the means of showing the transmission. Here is one of what are called Rupert's drops. It is a drop of glass cooled quickly, and drawn out into a long tail. This glass is in such a state that the slightest crack or rupture, such as the breaking off of the long tail, causes the whole thing to fly into powder. As I said, this rupture of the Rupert's drop is accompanied with a powerful mechanical shock. Here is a bottle of water (B, Fig. 41); I do not know whether it is

calculated to produce the effect. I want to show you not only the transmission of gentle pressure, but the transmission of a shock through water; and if I hold this Rupert's drop (D) in the bottle of water, and break off the tail, the

FIG. 41.

shock ought to be transmitted through the water, and if the experiment is successful the bottle will be broken. I will now break off the tail of the drop, and the bottle *is* broken to pieces. The breaking of the bottle is entirely due to the transmission of the force through the water.

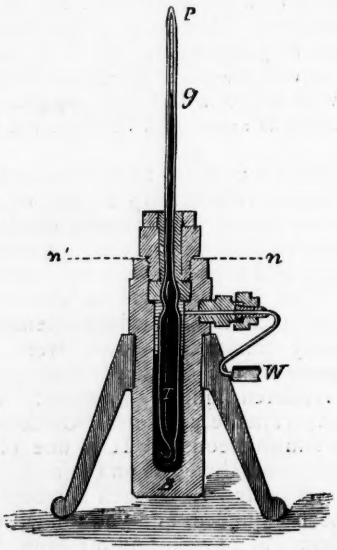
I introduce that experiment to your notice in order to make you understand another of far greater practical importance, and that is an experiment for which I am indebted to the obliging kindness of my friend Prof. Abel, of Woolwich. I have no doubt that it will have an important future. Here is a bombshell, and I will pour water into that bombshell: it is very thick. Here is a small charge of powder, and we will explode that charge of powder in the middle of that bombshell filled with water. We place the shell for protection in a strong metal case. The force will be sufficient to break the bombshell, and I should not like even that small quantity of powder to have its way in this room without carefully covering it up. Two electric wires lead from a small fuze which is placed in the centre of the powder, and we will fire the charge by means of the electric current, and if the experiment is successful the shock will be transmitted to the water and burst the bomb. Now I will press

that button and make contact with the battery, and you hear the noise of the explosion, and there you have the result. The bombshell is shattered to fragments, and reduced to that state by the force of the shock that has been transmitted to the water. I do not know what is to be the future of this form of bombshell, but I can see that it is of very great importance. Now calamities in coal-mines often occur by the use of gunpowder, and it is possible—I will not say that it is certain—that by surrounding our charges of gunpowder with water in this way, we can do away with the danger that now is associated with the use of gunpowder in mines.

I want to refer for a moment to the condensation of gases in so far as it bears relationship to that of vapour. You know that aqueous vapour is very easily condensed. It was long ago conjectured that the very air we breathe, and that oxygen, hydrogen, and nitrogen were the vapours of bodies, and that they could be congealed to a certain extent and reduced to a liquid condition. Various attempts were made, and various easily condensible gases were liquefied, but quite recently some of the more refractory gases have been condensed and rendered liquid by pressure. This has been done by two experimenters, Messrs. Cailletet and Pictet, working almost simultaneously. It is one of the cases in which we find the same thoughts on the same subject occurring to two different minds. Oxygen has been liquefied and nitrogen has been liquefied, and you know that the air we breathe is a mixture of oxygen and nitrogen. In 100 cubic metres of air there are 20 cubic metres of oxygen and 80 cubic metres of nitrogen. I will not take the atmosphere, or oxygen, or nitrogen, because these gases require for their liquefaction appliances which we have not the means of using here; but you remember the gas I brought before you in our first lecture,—the gas we obtained by the action of an acid upon fragments of marble,—carbonic acid gas. This gas I intend to liquefy in the apparatus which is now before you (Fig. 42). B is a strong iron bottle containing mercury, into which is fixed a thick glass tube, P T. The end of the tube at P is closed, the lower end being open. This tube is held firmly in position by a strong iron nut, which is screwed into the bottle at *n n'*. Before introduction into the iron bottle, the glass tube was filled with pure carbonic acid gas. The tube W, which opens into the iron bottle, is connected to a powerful hydraulic pump. We will now urge water, by means of this pump, through the tube W, into the iron bottle, and force the mercury into the glass tube

containing the gas. You see the mercury rising higher and higher as the pressure is increased, and it is now within a couple of inches of the top of the tube, at *g*, the whole of the gas contained in the lower portion of the tube being

FIG. 42.

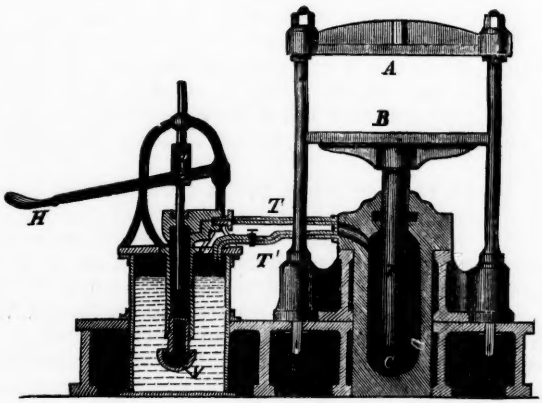



compressed into the small space, *g* P. Observe the top of the column of mercury, and you can see collected there a quantity of liquid, clear and colourless; that is liquid carbonic acid gas. By this compression we have actually changed the gas into a liquid. You see that it is actually boiling before you, even at this temperature. If I lower the pressure it will boil still more violently; the liquid will disappear, and again become a gas. If I increase the pressure the liquid gas is again formed. Now what I want you to observe is the effect of the sudden expansion of this atmosphere above the column of liquid carbonic acid. If you take a champagne bottle, say half empty, allowing the carbonic acid to fill the space above the champagne, and suddenly open it, you hear an explosion, and you find the space which was formerly perfectly clear above the liquid filled with cloud. That is due to the chilling by expansion which I illustrated a lecture or two ago. Now I will try to produce

that cloud in this apparatus; although it is composed of transparent carbonic acid, still it will appear almost dark for the time being. To produce this effect we must lower the pressure very suddenly; and observe, the instant I do so the space above the liquid is filled with a dense cloud.

And now I have to refer to an instrument which we employ in this experiment. It is what we call the hydraulic press. Its construction you will readily understand from this diagram (Fig. 43). A strong iron cylinder, C, with very thick sides, has inserted into it an iron ram, P, working water-tight in the collar of the cylinder. On the ram, P,

FIG. 43.




there is an iron table, B, on which the substance to be pressed is placed. Strong iron columns support an iron plate, A, fixed directly over the table, B. By means of a small pipe, T, the cylinder, C, which is filled with water, communicates with a small force-pump, P', which is worked by means of the lever, H. When the lever H is lifted, the piston P' ascends, the valve V opens, and the cylinder of the pump is filled with water. When the lever is forced down again, the valve V closes, and the water is forced through a valve in the pipe T into the cylinder C, and the piston P, carrying the table B, is forced upwards. By a repetition of this action enormous pressure can be obtained. When it is necessary to relieve the pressure, the screw plug inserted into the tube T' is opened, and the water flows from the cylinder C, through the tube T', into the cistern, and the

piston P descends to its original position. The principle of the hydraulic press is simply the forcing of water from a narrow cylinder into a wide one. We have here a small sample of the hydraulic press. It is the pressure of that which we have been working here which has converted the carbonic acid gas into the liquid form, and that without the exercise of very particular force by the human arm.

Now I want to try whether we cannot break a bar of iron by the hydraulic press. You have here no creation of power, but you have the means of applying power. You have the means of adding power to power, until, by the addition of small fractions of power, you are able to break that bar. We will place the arrangement for breaking this thick iron bar between the fixed plate A and the movable table B, and work the lever H as before described; the piston P rises, and now I have half a ton of pressure upon that bar, and you see it has snapped asunder. It has been placed across two supports, and a point pressed upon the centre; and by certainly not a very inordinate amount of effort on my part, I have been able to snap that thick bar asunder.

Now I wish to bring before you a most striking example —perhaps the most striking application of the hydraulic press that has been made in our time. The world is indebted to that illustrious engineer Sir William Armstrong for the introduction of the hydraulic press into a variety of engineering operations. This occurred to Sir Joseph Whitworth some years ago, and I had an opportunity of talking to him at the time, and I saw that the problem that he had placed before him was enormously difficult. Here I have a number of specimens of steel. This specimen has been cast as steel used to be formerly cast. It has been cut across, and you see that it is honeycombed,—full of bubbles of air, so that you never could trust steel of that kind. It might be very hard, very strong, but still you never could trust to it, because you never knew where these bubbles might not exist in the steel. Here is another sample of steel which is perfectly close, without the trace of a bubble. There is not a bubble large enough to admit a pin's head or a pin's point. It is perfectly compact. Now the problem which Sir Joseph Whitworth set before him was to convert steel of the old kind into steel of this close compact character. And how did he do it? He devised a mould of a certain quality,—a mould that would not allow the molten metal to pass through it, but would allow all these gases to pass through,—and he brought enormous pressure to bear

upon the steel when it was in a molten condition, and he produced what is certainly one of the greatest achievements of the time—this compact quality of compressed steel.

But here I am going to refer to one of the achievements of the hydraulic press. Whitworth places his steel under pressure and squeezes out every bubble, and he converts the steel from a material which is full of holes and unreliable into a steel of this compact character, which can be thoroughly depended upon. Now, thanks to Sir Joseph Whitworth, I am able to show you the manner in which he tests this steel. There is a hydraulic press, of very great power, invented by Sir Joseph Whitworth himself, and we will place a piece of Whitworth iron within the apparatus. The reason I wished to bring this before you was that I saw some time ago, in Great George Street, a pressure of 40 or 50, or 60, tons per square inch applied to the cylinder of iron that I now hold in my hand, and it was elongated and ultimately drawn asunder. The hydraulic apparatus is so arranged as to work by traction and exert a pull upon this cylinder of iron we are testing. This cylinder is exactly a square inch in area, and we will work the press, and exert an enormous strain upon it. It is worth while remembering what occurs. For a time it will resist the strain, and if we release the pressure it will go back again; but when the pressure goes beyond a certain point the iron passes what is called its limit of elasticity, and it begins to be elongated, and then after a time it snaps asunder. By the continuation of the strain on the iron cylinder we have reached a pressure of 20 tons to the square inch. This point is the limit of elasticity of the iron, and you see the cylinder begins to elongate, and is reduced in diameter from one inch to nearly a half inch; and now the cylinder has broken asunder. That has required a force of 32 tons to the square inch. The end of this cylinder is a square inch in area, and therefore it is as if you had screwed it up and attached to it a weight of 32 tons, which would be just sufficient to break the cylinder. That is called the strength of the cylinder; and here another point comes into play which is of very great importance, and that is the ductility of the cylinder. The ductility is measured in this way:—The power necessary to break a cylinder is measured by the number of tons required to cause it to give way: the ductility is measured carefully by placing the two broken halves together, and measuring the elongation of those two halves. That gives the ductility. The specimen we have just broken is a very ductile metal, but it is not of great strength. Here, however,

is a cylinder which is composed of Whitworth steel, which is enormously strong: we will place it in the hydraulic press, and subject it to the straining action. It may not at all succeed in the form of showing you the breaking of the metal, because the steel is so good that it may resist a force of 100 tons. There is a force of 10 tons on it now, and we shall go on augmenting the pressure: 80 tons are now working upon that cylinder; the gauge tells the story. You see what the heaping of small impulses will do—small increments of power. There is now a pressure of 100 tons to the square inch, and the steel has stood this test. That steel is not to be broken by that force.

And now, having said so much, I have only to offer you my best thanks for the attention you have given me throughout this course of lectures. I do not know that I have ever given a course of lectures in this place that has gratified me more; for I do not know that I have ever seen the young folks—the lads or the girls—more attentive than they have been during this course of lectures. I have to thank my excellent friend, Prof. Dewar; I have to thank Sir Joseph Whitworth; I have to thank Prof. Abel; and last, not least, I have to thank my excellent assistant, Mr. Cottrell.

# ANALYSES OF BOOKS.

*The Disestablishment of the Sun.* By JOHN BLAND. London: Sprague and Co.

IT has been asserted, perhaps in too boastful a strain, that Science knows neither sects nor heresies, and that her revelations are based upon such evidence as to render dissent altogether impossible. What, then, will be the sensations of our leading astronomers, physicists, meteorologists, and of the educated public in general, to find it boldly declared that there is no heat in the sun's rays? Such an assertion and others scarcely less startling are made in the pamphlet before us. Now we do not belong to that numerous and influential class who seek to establish a scientific orthodoxy, and who treat all dissidents with sublime contempt. We hold that he who looks at any question from a novel point of view may possibly see something worth recording, and that he who detects a flaw in our received theories gives us an opportunity not to be neglected. Still we must confess ourselves disappointed in this little work. We expected to have our attention called to facts hitherto overlooked, which the author had possibly discovered. We looked for experimental evidence. But our expectations have not been fulfilled. We must even doubt whether the author has in all cases taken the trouble thoroughly to apprehend the received theories which he is assailing, and the facts upon which such theories repose.

Mr. Bland's objects are—"To show the fallacy of the popular belief in the heat of the sun. To prove that the only heat we have on the surface of the earth comes from an internal source. To explain the primary function of vegetation. To explain the law under which trees develop. To show that at one time it is probable there was no water on the surface of the earth. To account for the formation of mountains. To show that it is probable that the quantity of water on the surface of the earth is still increasing. To account for the deposit of marine shells on hills. To account for the formation of coal strata. To show that it is probable that at one time no air existed on the surface of the earth. To explain the existence of heat under a burning-glass held in the rays of the sun, &c., &c."

Many of these explanations are of course perfectly unnecessary. The formation of mountains, the presence of shells not on but in hills, the formation of the coal strata, have long since passed out of the range of problems to be discussed. To say that at one time there was no water on the surface of the earth

is a needless truism, if, as Mr. Bland holds, in perfect accordance with the commonly received theory, the temperature of our planet was greatly in excess of that at which water can exist. To deny the existence of air is another question. Surely the higher the temperature the more matter must have been present in a gaseous state. Our author forgets, also, that no one seeks to account for the lower temperature of the poles than that of the Equator by their greater distance. The theory of direct and oblique rays, which he pronounces a "myth," is perfectly satisfactory. Suppose a number of parallel rays falling upon a given surface: the more such surface is deflected from a direction at right angles to the rays, the fewer of such rays fall upon each square foot of the surface. The author argues that if the poles are cold because the heat-rays fall upon them obliquely, a spot on the Equator at midnight ought to be colder still, because it is out of the sun's rays altogether! He forgets that at the poles the sun's rays are absent for one-half the year entirely, and fall during the other half very obliquely and scantily upon the soil; whereas at the Equator the surface of the soil has not time to cool down during a single night. This is, indeed, Mr. Bland's main error: he seems to think that if the sun is the source of heat, the temperature of any place should depend solely upon its being above the horizon, or not, at any given moment. In consequence of this mistaken assumption he asks—How is it that the temperature often gets higher in the night? and how is it there is not a great difference in the temperature when there is a total eclipse? The first of these questions is solved by reference to a change in the wind: as to the second, a chilly feeling has been observed during an eclipse; but the occultation is too brief, and extends over too small a part of the earth's surface, to bring on, *e.g.*, a frost. He asks, further, How is it that rivers become frozen on the surface rather than at the bottom? We thought it was now generally known that water expands in freezing, and the ice formed, being specifically lighter, floats upon the surface. Again, "How is it that the frost never penetrates more than a comparatively few feet into the earth?" Because earth is a very poor conductor. But if any appreciable quantity of heat were emitted from the interior of the earth, how is it that ice buried a few yards in the ground may be preserved during a whole summer, whilst if laid on the surface it melts in the first mild days of spring?

The author even asserts that the sun, so far from imparting, robs us of heat. He makes use of this hypothesis to explain away the effects of a burning-glass. He says—"If a burning-glass be held in the shade no change in the temperature is observed in its neighbourhood. If the shadow be removed an entirely different state of things is observed. The rays of heat that were lazily ascending *straighten out* and quicken, as if endowed with life. The evidence that they straighten may be

stated in a few words. It is known that the rays of light come straight from the sun. If a burning-glass be held so that it fairly intercepts the sun's rays, the foci of the concentrated rays of light and heat are identical under the glass. It is known that glass obstructs the passage of heat [!] and admits the passage of light. As the rays of heat and light must be reflected [should be, refracted] at the same angle to have the same foci, it is assumed [*i.e.*, by the author] that they are moving in the same straight line, but in opposite directions." Here is, in the first place, a grave error in fact. Glass does not obstruct the passage of radiant heat from incandescent bodies. Further, if Mr. Bland will take a powerful burning lens he will find that the intensity both of light and heat decreases *pari passu*, going from the focus towards the lens. This is what should happen if the light and heat move in the same direction, according to the common theory, and what should *not* happen if the light only passes through and is concentrated by the lens, whilst the heat arrives at the focus from the opposite side and is refracted and concentrated by nobody knows what.

We have not space to make any further extracts from this pamphlet. We think if the author would work for a few months in a good physical laboratory he would greatly alter his views. His manner of dealing with heat reminds us of Goethe's researches in optics.

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*Reports of the Mining Surveyors and Registrars of Victoria.* For the Quarter ending September 30, 1879. Melbourne: J. Ferres.

The total quantity of gold raised in the colony during the quarter has been 189,648 ozs. 14 dwts.

An Appendix by the Government botanist, Baron F. von Mueller, consists of observations on new vegetable fossils of the auriferous drifts.

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*Guide to the Geology of London and the Neighbourhood.* By W. Whittaker, B.A., F.G.S. London: Longmans and Co., and E. Stanford.

This pamphlet is an explanation of the Geological Survey-Map of "London and its Environs," and of the Geological Model of London in the Museum of Practical Geology. The author points out two errors: that the "London Basin"—so-called—is deep

in proportion to its length and breadth, and that the Tertiary beds have been deposited in a hollow cut out in the chalk. Mr. Godwin-Austen inferred from the exposures of older rocks in the Ardennes (Belgium) and in the Mendips (Somerset) that these are parts of one grand line of elevation, connected underground nearly along the valley of the Thames. His conclusion has been since verified by borings, but the direction of the dip of these strata has not yet been ascertained.

The author considers that no important supply of water need be expected on tapping the Lower Greensand, on account of its thinness and its very limited outcrop.

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*Records of the Geological Survey of India.* Vol. xiii., Part 1. 1880.

This issue contains the annual summary of the Survey and of the Geological Museum at Calcutta, for the year 1879. It is remarked that Mr. Griesbach, in examining the higher Himalaya of Kumann and Hundes, has discovered and brought in a good harvest of fossils.

We note with interest that two natives have received permanent appointments as Sub-assistants. The author seems, however, to doubt whether they can ever prove competent for independent field-work, which requires "the very quality which more than any other makes the western man differ from the eastern."

In the list of societies and institutions with which the Survey exchanges publications, we find the "Geological Survey of New Zealand" and the "New Zealand Institute" described as at Washington!

The memoirs comprised in this number are—"Additional Notes on the Geology of the Upper Godavari Basin," by W. King; "Geology of Ladak," by R. Lydekker; "Teeth of Fossil Fishes from Ramri Island and the Punjab," by R. Lydekker; "Notes on certain Fossil Genera in the Palæozoic and Secondary Rocks of Europe, Asia, and Australia," by Ottokar Feistmantel; "Notes on Fossil Plants from Kattywar, Shekh Budin, and Sirgujah," by O. Feistmantel; "On Volcanic Foci of Eruption in the Konkan," by G. T. Clark.

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*Geological Survey of Canada.* Report of Progress for 1877-78. Published by authority of Parliament. Montreal: Dawson Brothers.

THIS report, as usual, contains a goodly store of interesting matter. Considerable attention has been paid to the apatite beds, and a very large number of samples have been analysed. The mineral is decidedly rich in phosphoric acid, averaging about 40 per cent. The carbonates are trifling; iron is in many cases absent altogether, and alumina ranges from 0·267 to 1·969 per cent. The only drawback is the presence of fluoride of calcium, averaging 7 per cent. This constituent requires special precautions to prevent nuisance when the apatite is used in the manufacture of superphosphate. It may be pronounced decidedly superior to the Nassau apatite, which contains less phosphoric acid, more iron and silica, about the same fluorides, and alumina. To what extent these samples are fairly representative of the mineral veins seems, however, an open question. The apatite occurs in Ottawa county, and is accompanied by a great variety of mineralogical species, mainly similar to those found associated with apatite in Norway.

The gold deposits of British Columbia are described at some length. The precious metal is found both in alluvial deposits and as auriferous quartz. Near Kelley's lake a quartz vein has been discovered yielding 1·21 ounces of gold and 2·43 ounces of silver to the ton. Pellets of native silver have been found on the Similkameen, and the ore at Cherry Creek has been found to contain 658 ounces of silver per ton. On the South Similkameen the gold is mixed with coarse scales of platinum. Rich copper-ores and native copper have also been met with, but pending the possible discovery of deposits yet unnoticed the mineral resources of British Columbia do not seem of capital importance.

Cape Breton is rich in fine marble, which at present lacks a market, being excluded from the United States by a protectionist duty of 50 cents per cubic foot. Many of the beds are perfectly fit for statuary purposes.

The palæontological observations are not remarkably important. The Tertiary strata of British Columbia have yielded fossil insects in good preservation, but presenting few points of interest.

The Dominion must undoubtedly have many secrets, geological and biological, which are yet to be brought to light, and we must hope that the Survey will not on any account be cut short in its operations.

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Several important books stand over for review, on account of the want of space.

# CORRESPONDENCE.

*** The Editor does not hold himself responsible for statements of facts or opinions expressed in Correspondence, or in Articles bearing the signature of their respective authors.

## ON A POINT RELATING TO BRAIN DYNAMICS.

*To the Editor of The Journal of Science.*

SIR,—The following letter "On a Point relating to Brain Dynamics" (referred to by me in the article on "Natural Science and Morality," in your last number) might possibly have some interest as a mode of reconciling *Free Will* v. *Necessity*, said to be similar to that proposed by the late Prof. Clifford in an oral lecture, at St. George's Hall, on "Body and Mind." I am not aware if this lecture has been published, and the views in the subjoined letter were of course arrived at quite independently.—I am, &c.,

S. TOLVER PRESTON.

*From "Nature," May* 13, 1880.

ANY attempt to grapple with the doctrine of Free Will *v.* Necessity on the old lines would probably (and deservedly so) not attract much attention. The object of this paper is to place a consideration of extreme simplicity under critical notice, which would seem to be capable of affording a key to the complete reconciliation of the divergent views on a common basis; and since the matter to be dealt with will be strictly within the domain of natural science, a clear analysis will be rendered possible.

It is well known that the only attempt to harmonise the doctrine of Free Will with the principle of the Conservation of Energy consists in supposing that living creatures have a power, by the mere exercise of their "will," of deflecting particles of matter within their bodies from their natural paths, without thereby altering the total energy of the particles.* This, there-

* The necessity for this special assumption, in order to prevent Free Will from coming into direct collision with the principle of the Conservation of

fore, it will be observed in the first instance, assumes a peculiar physical state of things to exist within the body of an animal which does not prevail elsewhere, or it supposes that the laws of Nature have not a general application, but that the animal world must be made an exception. This at the very outset evidently involves a very questionable admission. My purpose is simply to point out that by taking into account a special consideration based on the evidence of modern physiology as to the functions of the brain, such an assumption as the above is rendered entirely superfluous, and that even if it could be supported it would still miss the main object in view.

Whatever room for speculation there may be as to the exact nature of the mental faculties, it is at least very generally admitted that these faculties are most intimately *connected* with or dependent on brain structure. Modern physiological research has at least placed this fact beyond question, or it is allowed that the mental faculties have at all events *a physical side*. From this it must follow, therefore, that what we call "identity," character, or individuality (as involved in "mind") must be dependent on the special structure of the brain; indeed this view is so widely prevalent that it becomes almost superfluous to insist upon it. Now it may be safely assumed that no upholder of Free Will would wish for more than that a person should act in strict accordance with his identity or individuality, for the object of Free Will certainly is not to annihilate individuality (or those personal *traits* which constitute character). But is not this precisely what would occur if this contention for a mysterious power of deflecting particles within the body could be carried out? for the effect of this contention would be to make the brain superfluous as a directing mechanism, which would be tantamount to abolishing it (together with the individuality, of which it is the seat). For where would be the use of the elaborate mechanism of the brain for directing the movements of the body if we are to have power of carrying out this same object by deflecting particles by "volition" (whatever that may mean)? This would be to substitute for the brain, with which the identity is bound up, the empty nothing "volition." In that the brain directs the corporeal movements; the identity, or that which constitutes the very essence of individuality, thereby directs. What more would we have? Attempt to supplant the brain by the vague notion "volition," and the individuality ceases to exist; or that very end is attained which those who support Free Will most wish to avoid.

From the very fact that the brain is *known* to exist, it therefore should be perfectly conceivable (if not even *à priori* a natural

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Energy, is so obvious that it will probably be regarded as superfluous to give references to particular authors.

conclusion) that the brain might be a mechanism competent to regulate all* the motions of the corporeal system (for a set of dynamical conditions adapted to *any* effect is conceivable). In view of this, does not the assumption of this mysterious "deflecting power" seem all the more unwarrantable, or even absurd; as if it were imagined that the brain, being already there to direct the corporeal movements, something additional were necessary to direct the brain, or as if it were supposed that [the brain being the seat of the identity] something besides the identity were required to direct the actions of the body? This would seem to be no more than a specimen of the kind of incongruities which may be expected to present themselves by any attempt to evade physical principles.

It could not, however, be said that the opposite party were entirely free from error. For there appears to have been a notable oversight on the side of those who uphold strict Causal Sequence in Nature (sometimes called "Necessity") in failing to appreciate adequately the important influence (on the question of Free Will) of the fact that the brain is the seat of individuality, as above insisted on. For the omission to give due import to this fact has naturally made strict Causal Sequence to appear as a sort of grinding process, whereby man's actions are determined *independently* of his individuality; a view which is no doubt repulsive, and may have served as some excuse for the invention of the curious device of deflecting particles by the "mind" or "will." It will be observed, however, that by simply substituting the word "brain" (which includes "mind") for the word "mind" in the foregoing sentence, a deflection of particles of matter (represented by the direction of material operations by the brain) then can take place in accordance with, and not in opposition to, the laws of Nature. For from the very fact of the brain substance forming part of the material universe, it must of course influence and direct material operations in conformity with natural causes.

Could it be justly said that there is any *compulsion* in this? Can there be compulsion in being obliged to act in accordance with one's individuality or identity (determined by brain structure), since the only conceivable escape from this would be to act *in opposition* to one's identity (scarcely a desirable end)? But, it may be argued, there is still some coercion left here,

* Does it not seem a violation of principle, or a kind of inconsistency, to recognise that the brain does, in fact, direct certain motions of the corporeal system (and even those of a complex character, such as the digestion of the food, the circulation, &c.), and yet to assume that the brain would be incompetent to direct *all* the motions of the body? It may be said that a reasoning process accompanies the direction of some of these motions, but not others. But then is not reasoning itself a brain process, or is it not universally admitted that the reasoning faculty (whatever its exact nature) is at least *connected with* the brain, or has a physical side, just as, indeed, the mental faculties generally (or "mind") could not exist without brain?

because, although brain structure may be the seat of individuality or "mind," nevertheless, since our brains were originally formed by the operation of causes beyond our control, there is coercion in this aspect of the case. But then do even the most ardent supporters of Free Will ever dream of upholding the expectation that an individual should have a control in the original formation of his brain? or do they not concede (and rightly) that the ideal of Free Will is that an individual should act in strict accordance with (and not in opposition to) his own identity? Yet this is precisely what the believer in strict Causal Sequence, who has a just appreciation of the functions of the brain, will maintain must necessarily occur. Solely in virtue of the fact that there is strict Causal Sequence in Nature are the actions brought into strict conformity with individual brain structures (or with identity). If the principles of dynamics were not rigid, or if the laws of Nature were liable to alteration, a man's actions might sometimes be in harmony with his brain structure, sometimes in discord with it; or any number of persons, though possessing totally different brain structures, might act identically. The questionable expediency of the proceeding of those who are disposed to grumble at what they term the "iron" laws of Nature becomes apparent here.

But is it not, after all, more satisfactory to look to a definite physical basis for identity or individuality, as dependent on the magnificent mechanism of the brain, in preference to the superficial view of ignoring all this? No doubt there have been misunderstandings on both sides of this Free Will *v.* Necessity question,—the Free Will party, failing to appreciate justly the sequence of cause and effect; the Necessitarians, on the other hand, omitting to realise fully the important bearing of the relation of individuality to brain-structure on this question. No logical ground could be given why a complete agreement should not be possible on this subject; for there can evidently be but one correct view on any subject or question whatever. Moreover, from the very fact of the fundamental character of this question, it would follow necessarily that the wrong view on this subject must involve a great error, which therefore could hardly escape detection under a careful analysis. The divergence of views here is, however, no doubt more apparent than real; for if Free Will may be justly regarded as the freedom to act in accordance with identity (or as the assertion of individuality), then such freedom of will actually exists, and, moreover, the very condition for its existence is seen to be the prevalence of that strict Causal Sequence in Nature demanded by the Necessitarians. Thus the two views would show themselves capable of reconciliation on a common basis. That this fact should have apparently hitherto escaped appreciation may possibly be to some extent due to that spirit of partisanship which has so largely entered into this question, whereby the judgment may be allowed to be uncon-

sciously biassed, so that in some cases, instead of searching impartially as to what *is* truth, the inquiry has perhaps rather been as to what *ought* to be truth.

S. Tolver Preston.

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## STRANGE PROPENSITY OF CATS.

*To the Editor of the Journal of Science.*

Sir,—Perhaps you, or some one of your correspondents, may be able to throw a light on the attraction which the pretty blue-flowering annual *Nemophila insignis* seems to exert upon cats. They roll on it, nibble it, and in short maltreat it in such a way that its cultivation in a suburban garden is, according to my experience, impossible. I should like to know whether this circumstance has been observed by others, and whether any experiments have ever been made with a view of explaining the matter.—I am, &c.,

Exeter.

[The fact noticed by our correspondent was discussed some years ago in "The Zoologist," and it has repeatedly come under our personal observation and been mentioned to us by friends in various parts. We are not aware that the plant has ever been chemically examined.—Ed. J. S.]

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## INSTINCT AND MIND.

*To the Editor of the Journal of Science.*

Sir,—At an early opportunity I shall crave your kind permission to make a few remarks on Mr. S. Billing's interesting article, which appears in your July number.—I am, &c.,

R. M. M.

# NOTES.

Mr. J. W. Mallet, F.R.S., in a communication to the Royal Society, shows that although hydrogen is occluded by aluminium its quantity is too small to vitiate the determination of the atomic weight of that metal.

Among other papers recently read before the Royal Society are—"Notes of Observations of Musical Beats," by A. J. Ellis, F.R.S., &c.; "The Aluminium Iodine Reaction," by Dr. J. H. Gladstone, F.R.S.; "On the Critical Point of Mixed Vapours," by James Dewar, F.R.S.; "Experimental Researches on the Electric Discharge with the Chloride of Silver Battery," by W. De la Rue, D.C.L., F.R.S., and Dr. H. W. Müller, F.R.S.; and on the Lowering of the Freezing-point of Water by Pressure," by James Dewar, F.R.S.

Prof. E. Ray Lankester has communicated to the Royal Society a note on the discovery of a fresh-water Medusa, of the Order Trachomedusæ, in a tank in the water-lily house of the Royal Botanical Society. It has received the name of *Peregrinella Sowerbii*.

Prof. E. A. Schäfer, F.R.S., and Mr. F. A. Dixey have laid before the Royal Society a preliminary note on the ossification of the terminal phalanges fo the digits. The process begins at the tip, and not at the centre.

Prof. W. C. Williamson, F.R.S., in a paper presented to the Royal Society, gives a very interesting account of the organisation of the fossil plants of the coal-measures.

Prof. Hoppe-Seyler has obtained from the alcoholic solution of chlorophyll crystals of a body which he names chlorophyllon, and to which he ascribes the decomposition of carbonic acid under the influence of light, and the peculiar fluorescence of chlorophyll. On treatment with caustic alkalies at elevated temperatures a purple-red compound is obtained, dichromatic acid, which possesses very remarkable optical properties. — *Zeitschrift Physiol. Chemie*, iv., p, 193.

According to the "American Naturalist" the English sparrow has become such a nuisance in Iowa that the State legislature is considering plans for its extermination.

From the same journal it appears that the arrival of migratory birds from the south is determined by hot winds lasting about three days.

Among eminent scientific men recently deceased we must mention Prof. D. T. Ansted, F.R.S., and Count de Castelnau.

According to a memoir communicated to the Academy of Sciences, the equatorial and polar diameters of the planet Mars agree with the hypothesis of an anterior fluidity.

M. Schützenberger, ex-Professor of the University of Strasburg, gives, in the "Revue d'Anthropologie," a curious account of a dog which was in the habit of stealing carrots, not of course for its own use, but to give to a horse—*un grand diable de cheval*—for which he had a particular affection.

Since the commencement of the present year the deaths at Paris have exceeded the births.

We hear with great pleasure that the manufactory of spurious university diplomas at Philadelphia has been broken up. Several barrelsful of fraudulent medical diplomas, just ready to be issued have been seized, and five "bogus" colleges, are in danger of suppression.

According to the "Medical Press and Circular" a belief is spreading in Hampshire that the luminous properties of Balmain's phosphorescent paint are due to an admixture of human fat!

It is not generally known that the superstitious practice of hoplochrism still prevails in Suffolk. If anyone injures himself with a tool or weapon he is at once exhorted to apply some healing ointment, not to the wound, but to the blade or point. The belief that stones are capable of growth is also still entertained in the Eastern counties.

The Birmingham Science College will open on October 1st, Prof. Huxley delivering an inaugural discourse. The course of Biology will be conducted by Prof. T. W. Bridge, F.Z.S.

Mr. J. S. Kingsley, writing in the "American Naturalist," pronounces the American Association for the Advancement of Science "a drag rather than an aid to progress." He adds that "aside from its grants of money to the Zoological and Geological Records, and to specialists, to enable them to carry out certain lines of investigation, its British prototype is no more worthy of its pretentious name."

Prof. Cope suggests that the mutilations and strains which plant-using animals have for long periods inflicted on the flowering organs may, as in some similar cases in the animal kingdom, have originated peculiarities of structure.

According to R. E. Kunze, in the "American Naturalist," the potato-beetle (*Doryphora decemlineata*) is poisonous to man. The usual result from handling the crushed beetles, as well as from inhaling the fumes arising from vessels in which they have been scalded, has been likened to serpent and scorpion poisoning.

Where death has followed, the blood has become disorganised the same as from septicæmia. (Can the poisonous principle be cantharidine? In America this question would admit of an easy answer.

It has been pointed out that the use of palls and mourners' cloaks at funerals furnishes a very efficient medium for the spread of infectious diseases, and their use has on that account been formally abolished among the Jewish community.

A writer in "Baily's Magazine" makes some very curious statements concerning the physiological action of different alcoholic liquids, if taken in excess. Good wines and malt liquors cause a man to fall on his side; whiskey produces a fall on the face; and cider and perry throws the subject on his back! To decide this question by experiment would, we suppose, be considered vivisection.

Abstinence from pork is no safeguard against trichinosis. Two French soldiers have lately died of this disease at Thionville, in consequence of having partaken of the flesh of geese. Trichinæ have also been detected by Dr. Clendenin in a pike caught near Ostend.

If the future career of the Manchester University bears any relation to the class of persons who figured at its formal inauguration, Science will occupy but a very subordinate position.

According to Dr. Vincenzo Peset y Cervera the crystals of hæmoglobulin obtained from the blood of different animals have forms so distinct and characteristic that the origin of a sample of blood may thus be determined. All that is required is to mix the blood with a little bile, when crystals not exceeding 0·003 metre in size are formed in the mass. The shapes of the crystals are said to be as follows:—Man, right rectangular prisms; horse, cubes; ox, rhombohedrons; sheep, rhombohedral tables; dog, rectangular prisms; rabbit, tetrahedrons; squirrel, hexagonal tables; mouse, octahedrons; &c.

[If these observations are confirmed they may serve for the solution of a most important question raised by Dr. Lionel Beale. If the theory of Evolution is true, the crystals obtained from animals which are nearly related should be either identical or such as are in form easily derived from each other. Should the hæmoglobulin crystals—*e.g.*, of the horse and the ass, of the dog and the fox, of the rabbit and the hare, or of the rat and the mouse—belong respectively to different systems, it will supply a serious argument in favour of independent creation.—Ed. J. S.]

According to Prof. Claypole, of Ohio, many wild European plants have migrated into America, and have there become so common as to prevail over some of the indigenous plants of the country. Only two or three American wild plants have crossed

the Atlantic and become naturalised in England. The difference of climates and the conditions of intercourse between the two countries do not furnish any satisfactory explanation of so marked a difference in the migratory power of the two floras.

According to Dr. Dujardin-Beaumetz the alkaloids of the pomegranate bark occasion paralysis of the motor nerves, whilst leaving the muscular contractility unimpaired. They do not affect sensibility, and may rank among the curarising poisons.

In a girl who died recently, at the Hôtel Dieu of Caen, the whole of the thoracic and abdominal organs were found completely inverted, the heart being at the right side, the liver at the left, whilst its usual position was occupied by the stomach and the spleen.

"Les Mondes" gives, from some source not specified, an account of a so-called electric girl, living at London, in Canada, whose hand cannot be touched save on penalty of an electric shock. She can give a violent shock to a chain of fifteen or twenty persons who join hands, and she has the power of magnetic attraction. Packets of needles, even if wrapped up in paper, hang suspended from her finger-ends. If she enters a room all the persons present undergo a perceptible influence; some are made drowsy, and others feel indisposed and enervated until her departure. A sleeping child awakes at her approach, but a slight caress with her hand sends it to sleep again. [How, when her touch communicates an electric shock?] Animals are equally influenced by her; a favourite dog of the family remains for entire hours at her feet, as motionless as if dead. Very strange, if true.

Acccording to M. Bidard the plane tree (*Platanus*) exerts an injurious action on human health, sometimes to the extent of producing discharges of blood from the lungs. This action is said to be due to a white down which is formed on the lower surface of the leaves, and which is easily detached by the wind.

Mr. Tremlett, Consul at Saigou, gives particulars concerning *hwangnoa*, which is said to be an antidote for the bites of the most venomous serpents, and to have been used successfully in the treatment of cancer and leprosy, which is very common in Tonking.

A sum of £60,000 has been subscribed for the establishment of a college in Liverpool, on the principle of those of Leeds and Bristol.

A species of *Dasyurus* (*D. fuscus*) has been found in the Arfak Mountains of New Guinea—the first Australian carnivorous type found in that island.

MM. A. de Quatrefages and L. Hamy have communicated to the Academy of Sciences a memoir on the craniology of the various subdivisions of the Negro race.

The study of Anthropology is beset with difficulties. Even the skull of a savage killed in battle is begrudged to Science, and is made the subject of a "question" in Parliament.

A new poisonous alkaloid, of a pleasant odour, but of very deadly properties, has been discovered in the fumes of tobacco. It approaches closely to collidine.

An Acarus of the genus *Trombidium* has appeared in Vaucluse in the welcome character of a destroyer of the *Phylloxera*.

According to M. Dieulafait zinc can be detected in all rocks of primordial formation, and in the waters of the seas of all ages.

According to M. H. Filhol a number of new mammalian fossils have been discovered in the phosphatic beds of Quercy. Among them may be noticed *Quercitherium tenebrosum*, a carnivorous species approximating to *Cynohyænodon*.

M. F. Peuch has proved, by a series of experiments described in the "Comptes Rendus," the fact of the transmission of tubercular disease by the milk of the animals affected.

A writer in the "Druggist's Circular" (American) complains that the English sparrows introduced into the United States have lately (?) acquired a taste for destroying the buds of the peach, pear, and apple trees, and even of the garden pink. The editor in reply gives the good, but scarcely practicable, advice to apply poison to the buds.

The "army worm," often mentioned in American agricultural literature as very destructive, is the larva of a Lepidopterous insect. It has a peculiar propensity for destroying wheat.

We regret to learn that in the State of Missouri the office of State Entomologist, which has been most ably filled by Prof. Riley, has been abolished.

The evil effects of opium smoking appear on closer examination to have been greatly exaggerated. Of the 17 million lbs. of opium yearly consumed in China, 5 millions are of native growth.

Dr. Sylvester Marsh, jun., has communicated to the Quekett Microscopical Club his experience of the processes used in bleaching and washing microscopical sections intended to be stained. The objection to alcohol as a bleaching agent is that it is very slow in action and uncertain in result. Chloride of lime and Labarraque's solution of chlorinated soda are liable to disintegrate and destroy, unless very cautiously used. Dr. Marsh prefers euchlorine. The apparatus consists of two of the bottles in which an ounce of quinine is usually packed: one is used as the gas generator, from which a bent glass tube passes nearly to the bottom of the second, containing water, in which the sections

to be bleached are placed. In the first bottle a sufficient quantity of crystals of chlorate of potassa is placed to cover the bottom, and about a drachm of strong hydrochloric acid is poured upon them. The gas, saturating the water, safely and effectually bleaches the sections. A notch is cut in the cork of the second bottle, to allow the escape of the gas when the water becomes supersaturated. The process is conducted out of doors, and exposure during the night is sufficient to bleach the sections. The washing is conducted in a bottle of the same kind. A funnel passes through the cork, and is continued to the bottom of the bottle by means of a piece of india-rubber tubing. A small hole is drilled in the side of the bottle, about half an inch below the shoulder. The sections are placed in water in the bottle, a filter-paper is placed in the funnel, and the whole placed under a tap arranged to allow of a suitable flow of water. The washing will be found complete after a night's exposure. In the discussion which followed Mr. W. H. Gilburt remarked that the bleaching and staining processes were not altogether satisfactory, because, if the relation of the tissues to each other was to be observed, it was necessary to do something more than bleach them, and when cell-contents had to be examined it was necessary to do so in their natural condition, and the tissues should be as fresh as possible. Dr. John Matthews said there was a mode of differentiation of tissues which was too often overlooked, and that was the use of the polariscope. If the object under examination did not yield to one selenite, it usually would to another.

Mr. F. Barnard, of Kew, Victoria, writing to "Science Gossip," recommends the use of carbolic acid in place of turpentine for preparing objects for mounting in Canada balsam. It has the advantage of not rendering tissues brittle, as is the case with turpentine, and allows objects to be mounted in balsam without previous drying.

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ERRATUM.

Page 406, line 13, *for* "coon" *read* "born."